

REVIEWS.

ART. XIX.—*Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session, held in Philadelphia, May, 1852*: Vol. II. Published by the Society: 8vo. pp. 146.

THE Medical Society of the State of Pennsylvania has evinced, certainly, a commendable spirit of industry. Though but recently organized, and, as yet, comprising but a small portion, comparatively speaking, of the physicians of our State—the majority of the counties being unrepresented in its councils, and lending no aid in the furtherance of the great work in which the Society is engaged—it has, nevertheless, succeeded in directing the attention of our medical men not only to a more diligent cultivation of the several fields of inquiry within their reach, but to the importance of communicating the results of their labour in a form calculated to augment the general stock of professional knowledge.

The physicians of Pennsylvania have much reason to be satisfied with the two volumes of *Transactions* that have been issued by the State Society. The reports of which they are composed are not, it is true, replete with profound inquiries tending to the elucidation of either of the mooted questions in the etiology, the pathology, or the therapeutic management of disease; they are, more generally, a simple record of detached observations, made by men constantly engaged in the active duties of their profession, often under circumstances and in situations that leave less time for prolonged and elaborate investigations, and for the presentation of their results in a shape adapted for the press, than those who practice the profession in the midst of the dense population of a large city are generally aware. Many of the observations recorded in these reports may, perhaps, be considered trivial and commonplace; still, as additional evidence of the correctness of generally received pathological or practical views, derived from the experience of different observers in dissimilar locations, they have an important influence in giving permanency and increased force to medical truths that are, most generally, of everyday applicability.

The volume of *Transactions* for 1852 contains reports from eleven counties. Two leading subjects appear to have occupied the attention of the majority of the medical societies from which these reports emanate. The first of these is a geological survey of the several counties; and the second, the solution of the question, whether particular diseases or classes of diseases have any positive connection with distinct geological formations.

Three of the County Medical Societies have furnished very beautiful and accurate maps of their respective counties, to illustrate the general description of their geological features as given in the reports. Most of the other reports contain partial geological descriptions of the counties of the disease of which they treat; that will, we have reason to believe, be rendered more complete, and illustrated by maps hereafter.

In respect to the second subject, the relationship between certain diseases and particular geological formations, the materials as yet collected are not sufficiently extensive to lead to any positive results. The question is still to be

determined by a series of similar observations, made in the different sections of the State where the same geological features are met with, and extending through a number of years. Much credit, however, is due to the County Medical Societies of our State for having commenced the inquiry, and we would urge them to prosecute it faithfully until some satisfactory conclusion shall be attained. Enough has been already done to show the importance of studying the geology of a district in our investigation of the etiology of its endemic diseases. As was correctly observed in the report of the Berks County Society, presented last year:—

“In the present state of science, every topographical description must be deemed imperfect that has not geology for its foundation, and topography is manifestly a necessary element in the history of disease.”

The disease, which appears to have been most extensively studied in reference to the geological formation of the different districts in which it occurred, is dysentery. Some of the statements in reference to its greater prevalence in gravel, slate, and alluvial formations, and its rare occurrence in limestone regions, are merely negative; while others would appear to indicate that, under particular local and meteorological conditions, it is as liable to occur upon one geological formation as another.

In the report from Delaware County, it is stated that—

“After a careful examination and comparison of facts, the committee are impelled to the belief that the diseases of this county during the year have not been in any manner influenced by geological formation, except that intermittent and remittent fevers have been confined to the marsh lands on the Delaware River.”

We are to recollect that Delaware County has but very little limestone. The rocks are mostly mica slate, gneiss, and sienite, though there are also considerable quantities of granite, quartz, felspar, mica, and serpentine. A considerable portion of the county is covered with drift, from a few inches to several feet in thickness, composed chiefly of silicious sand and pebbles. Along the Delaware River, there is a deposit of alluvium, varying in breadth from half a mile to a mile; a considerable portion of it is marsh, which is generally well banked and drained.

The committee, in the report from this county, remark that

“*Dysentery* has been much more common in all parts of the county than for many years before, and in some places has been quite malignant and unmanageable.”

In the report from Lebanon County, it is stated that

“The diseases prevalent in this county present no marked peculiarity, nor has the nature of the soil, to a great extent, any influence in the production or propagation of disease. In the gravel district, *dysentery* prevails more frequently than in other parts of the county, either endemically or epidemically, and often in the most malignant form. Whether the distinctive geological features of this section may furnish the cause of the frequent occurrence of this malady, is a question which has not yet been solved. The surface of a large portion of the district, the slopes and tops of the ridges, being composed of shaly and shity materials, is not raised to an elevation exceeding that of our limestone valley. The shales are of a ferruginous composition, interspersed with specks of sulphure of iron, and contain other mineral salts to a much greater extent than occur in the limestone regions. The water of the wells and springs, in the summer and autumnal months, when the supply of it is much less than at other seasons of the year, is probably more charged with mineral salts than when the flow of water through the interstices of the slates is greater in a given time, and to this may be attributed the exciting cause of this complaint. If such be the

case, it has been suggested that filtering the water through finely-pulverized limestone would answer as a preventive of this flux."

In the report from Mifflin County, we find the following statement:—

"The assertion, in the Berks County report of last year, that *dysentery* never prevails epidemically upon the limestone, is not borne out by our experience here. Many of the cases occurred upon the lime rock, and amongst those who used the hard water issuing from it."

Logan's valley, Blair County, has principally a limestone soil; the soil of the hills, as well as of the Alleghany Mountain, rising abruptly from among them, on the east side of the valley, is chiefly slate and red sand. Throughout this valley, *epidemic dysentery*, we are informed by Dr. Confer, extended with great violence during the latter part of the summer of 1851, only disappearing on the approach of cold weather. "It occurred," he adds, "after a dry and warm season. All the ponds and stagnant pools were dried up, and the streams were generally low."

Dr. Rodriguez, of Hollidaysburg, Blair County, states that, in his vicinity, he has "witnessed *dysentery* both in its epidemic and sporadic visits: twice as a well-marked epidemic, in 1832 and 1842, and, if the number of cases that it fell to his lot to prescribe for in 1851 be considered, when added to those of our neighbours, as sufficient to constitute an epidemic, then he has seen it three times."

The localities in which it prevailed he describes as "the Alleghany Mountain, secondary formation, Hollidaysburg, transition limestone; most extensively in the former."

Dr. C. W. Parrish, of Marshaltown, Chester County, remarks: "The *dysentery* has frequently been prevalent (since 1828) as an epidemic, but was generally confined to the elevations, and principally to the talc-slate range, though cases occasionally occurred in the regions previously occupied by fevers."

The remarks which follow in immediate connection with the above statement are particularly interesting.

"In July, 1850," says Dr. Parrish, "a considerable flood occurred, which caused the Brandywine to overflow its banks and to submerge the meadows, at a time when they were either mowed, with the hay remaining on them, or having a very heavy growth of grass ready for the scythe. The mass of vegetable matter passed with unusual rapidity into a state of decomposition, and the effluvia became extremely offensive. From one meadow on the east branch, the stench was carried, by the prevailing currents of wind, a considerable distance up a hollow to the house of one of our most substantial farmers, where, in the month of August, the *dysentery* attacked, in succession, nine members of the family, leaving only three unscathed; and also made its appearance in several other families living near the meadow, while other houses, not within the range of the prevailing winds, escaped its visitation. About six miles above this place, the flood brought down, and lodged in fence corners, and high against the trunk of trees, very many tons of recently mown grass, and of hay nearly ready for the barn. Late in the fall, that locality was visited by remittent fever of a rather typhoid character, from which the patients recovered very slowly, but which was confined to three or four families living on the banks of the stream."

"During the summer of 1851, *dysentery* appeared in the neighbourhood last referred to; and in a distance of about a mile along the creek, I believe hardly a family escaped its ravages; and the proportion of deaths was very great."— "I have perhaps 'travelled out of the record,' in alluding so minutely to the diseases of 1850; but I was led to do so in order to call attention to the fact that, in that year, the same local causes, in one vicinity, produced *dysentery*,

in another fever; and that, in the succeeding year, the locality previously visited by dysentery remained healthy, while the ground occupied by fever in the preceding year was the seat of a most violent dysentery."

Dr. Thomas, of West Chester, Chester County, says that he thinks he has "more frequently met with dysentery, as an epidemic, on the slate ridge north of the borough than elsewhere, though it has so occurred, occasionally, on the low ground." Subsequently, he states that "he saw, last summer (1851), a few cases of dysentery, probably twenty-five or thirty, the peculiarity of which appeared to him to be that they prevailed more upon the low than upon the high grounds. During the period that he has practised his profession, he has generally met the disease more on the high grounds; but most of the severe cases of the past season were on the low, and near streams of water."

Dr. P. A. Ogier, who resides in the great Limestone Valley, in the Township of East Whiteland, makes the following report:—

"The most serious epidemic of the year (1851) was *dysentery*, which prevailed so extensively over a large portion of this and the adjoining counties."—"Early in July, the disease appeared on the Slate Ridge, to the south of the Limestone Valley, and was confined to this region for some weeks, attacking a number of families living adjacent. In the month of August, it became more general, and I had cases in almost every quarter of my practice."

Further on, he says:—

"I could discover no local cause of the disease. Indeed, it appeared in almost every variety of situation: on the summit of the Slate Hills, in the rolling country to the south and east, as well as in the Limestone Valley."

Dr. W. D. Hartman, a resident practitioner of the borough of West Chester, writes as follows:—

"Of *dysentery*, twenty-four cases (all white) occurred in my practice. The cases varied from a mild attack, requiring but three or four days for its cure, to the most severe form of the disease. About half my cases occurred during August, and were confined to the mica slate ridge. The weather was very hot and dry, with cool nights; the difference of temperature between the mornings and evenings and the middle of the day, in the localities affected by the disease, as shown by the thermometer, varied from 25° to 30°."

Dr. J. P. Edge, of Downingtown, Chester County, remarks as follows:—

"Dysentery first made its appearance in this vicinity on the 1st of June. It continued until late in October. So widespread was it, and so general through some families, that the conclusion that there was some common cause was irresistible. My attention was directed to the detection of any appreciable one; but the disease was seen in such variety of locations as to baffle any definite conclusion. The weather was warm and dry to an unusual extent throughout the summer and early fall, affecting sensibly the springs and wells of the neighbourhood. I noticed, as in former seasons, that most of the locations visited were immediately sheltered by a hill (though not so invariably southern exposures as in former seasons), with a spring-head or streamlet near the door, *i. e.*, places where there was a high temperature, with great radiation and evaporation from the surface, and which were but little influenced by the winds from the north and west. Of forty-eight cases noted, five only occurred where limestone water was used by the family, although full one-half of the whole number resided on the limestone formation, or on its edge. Those five occupied situations where the sun's rays exerted great influence, and where there was a want of cleanliness and comfortable lodging. On this subject there has always prevailed a sentiment in this place, which can be traced back to the experience of Drs. Todd, Kersey, and Fairlamb, that the limestone water of our valley had a prophylactic effect against dysentery."

Dr. Edge remarks that, out of the first fifteen cases, twelve were in persons who used water that had been conveyed through leaden pipes, and he attributes the aggravated character of some of these cases to this circumstance.

Dr. S. H. Harry, of Doe Run, Chester County, states his impression to be that congestive fever prevailed in the limestone districts and in the valleys; whereas, *dyseentery* prevailed mostly on the hills; and he has never known it to prevail as an epidemic in limestone districts.

Dr. Griffith, of Springfield, Chester County, after enumerating pneumonia typhoides, *dyseentery*, bilious, remitting, and intermittent fevers, smallpox, varioloid, measles, hooping-cough, scarlatina, chicken-pox, parotitis, catarrh, or influenza, erysipelas, and rheumatism, as the diseases which have prevailed in his district of country within the last forty years, adds:—

"I have not noticed that any of these diseases have been more liable to occur in one geological formation than another; but intermittent and remittent fevers are more apt to prevail in low situations and near swamps and stagnant waters."

Subsequently, he remarks:—

"Limestone does not abound in this region of country, and I have no experience of the preventive qualities of limestone water in *dyseentery*; but I will state a fact in relation to that disease. There are some six or seven families, and part of the time there have been as many as twelve or fourteen families residing in the buildings which are situated immediately on the bank on the west side of the dam at Springfield Forge. I have attended the greater part of all the cases of disease which have occurred at that place for forty years, and, although *dyseentery* has prevailed at various times, at nearly every house in the vicinity of that place, I cannot recollect of ever attending, or hearing of a serious case of *dyseentery* in any one of these families."

In the report from Bucks County, we are informed that—

"An obstinate and, in some instances, fatal *dyseentery* occurred in many situations, during the hot and excessively dry summer and autumn of 1838; in our practice, it was almost exclusively confined to an elevated sandy ridge, extending from the Neshamony Creek to the River Delaware, upon which Doylestown is situated, and which bounds the Buckingham and Solebury limestone valley on the north-west. We have no recollection of any case originating in the limestone valley immediately adjacent. This remark, however, conflicts with the observations of our medical brethren of Montgomery, in connection with the *dyseentery* that prevailed in the limestone valley of Plymouth and Whittemarsh, in the year 1840. The discrepancy we will not attempt to reconcile."

Dr. Beaver, in the report from Berks County, describes epidemic *dyseentery* as having prevailed pretty extensively in his neighbourhood. The cases which came under his own observation were of a mild character, and for the most part yielded readily to an ordinary simple treatment. The whole number of cases was seventy, of which five proved fatal. "The geological formations on which my cases occurred," he remarks, "were generally the limestone, and some seven or eight on the gravel, near the junction of the two. My past experience," he adds, "brings me to the conclusion that *dyseentery* generally prevails in a more unmanageable form on the gravel and slate formations than it does on the limestone."

In the same report, Dr. Bertolet, speaking of the occurrence of epidemic *dyseentery* in Oley, says:—

"The first case of *dyseentery* in my practice occurred on the 26th of July. The patient was my next neighbour. The disease immediately spread in every direction; although southwardly, where we have an entirely different geological

formation, it did not extend beyond half a mile."—"The season was unusually dry, the country being, in many places, almost parched."—"The disease did not abate until the cold weather set in, and indeed was somewhat aggravated by the first light frosts."—"The districts where dysentery was most common were either of the slate or gravel formations, or slate overlaid with limestone. Some few sporadic cases were found in the limestone region, but these could almost invariably be traced to exposure on gravel. I noticed that persons using the water of springs having a southern aspect were more liable to attacks of the disease than those who obtained it from an opposite locality. Every family residing on the southern inclination of the Monocacy Hill, so named from the creek which has its source in it, using the waters of the sparkling springs which in a great degree irrigate the soil, were, with one exception, seized by the disease, and even this one did not entirely escape, as it had it in a mild form, which I attributed to the fact that he drank water drawn from a deep well. On the contrary, the people living on the northern side were not attacked at all. Families, in general, using water from deep wells, or springs having a northerly exposure, were taken less early or severely. To these causes I attributed the immunity of my own family. My well is deep, well sheltered, and on the northern side of the house.

"These facts, as well as the prevalence of the epidemic during and after an unusual drought, prove that solar heat exercises a powerful influence, both in predisposing the system and developing malaria in the production of dysentery.

"It is not a little curious that those who had the disease two years previously, when it prevailed to some extent, escaped last season. Typhoid fever existed simultaneously with dysentery, but in no case was a patient attacked by both at the same time. In persons who had both in succession, sometimes one, sometimes the other preceded."

In the report from Huntingdon County, we find the following remarks:—

"As to the influence which different formations of soil may exercise upon disease, or, in other words, whether, for example, the slate formation will increase the progress and severity, or protract the removal of diarrhoeas and dysenteries more than the limestone formation, is a matter difficult to decide in this mountainous country. In the fall of 1849, *dysentery* prevailed with much violence in a limestone district in the north-eastern part of the county, while the neighbouring slate formation district, with a few exceptions, remained free from that disease. Again, it is an everyday occurrence to hear persons who are accustomed to the use of limestone water complain of pain in the bowels and diarrhoea, after having taken a few drinks of slate water away from home. It would appear that atmospheric influences are the most obvious causes of disease in this county. Hence we consider the sudden transitions from warm to cold, and from cold to warm, and the malarious miasma which exists endemically through the whole valley of the Juniata, the main causes of the most of our maladies."

We have thus brought together all the statements contained in the reports comprised in the volume before us, having a direct bearing upon the question of the geological relationship of dysentery—the only disease in regard to which such relationship has attracted special attention. It must be evident that the observations upon which these statements are founded are too limited in extent, and in many instances have been too loosely made, to enable us to arrive at anything like a satisfactory conclusion. They are, however, in more respects than one, interesting, and, by directing the attention of the physicians of our State to the accurate investigation and careful record of all the circumstances, local or general, telluric or meteorological, domestic or social, under which the epidemic and endemic diseases that prevail in the different sections of it occur from year to year, they must necessarily lead to important results. So far as the question of the relationship of disease to particular geological formations is concerned, if the plan suggested for the appointment of commit-

tees charged with the investigation of the diseases prevailing throughout the several geological divisions of the State, be adopted, by the subsequent comparison of all the facts obtained by these several committees, some positive results may be anticipated.

In one or two of the reports, reference is made to the occurrence of erysipelas as an epidemic, and its connection with puerperal fever is either directly or indirectly noticed:—

"Erysipelas," we are told, "in the latter part of March, 1852, made its appearance in Palmyra, Lebanon County, and its vicinity, as an epidemic, and proved fatal in many instances. Few lying-in women escaped its attack, and the ratio of mortality (in these), we have been informed, was quite large. It has thus far confined itself to this section, included within the red sandstone and silurian limestone."

Dr. Rodriguez, of Hollidaysburg, Blair County, states that—

"Erysipelas has prevailed to a considerable extent this last year (1851); it generally assumed an inflammatory character. The antiphlogistic course was pursued, and the only external applications have been either a bread and water poultice (properly made) or a solution of acetate of lead for the first forty-eight hours; then the inflamed parts and sound skin were painted twice or thrice a day with tincture of iodine. Should we unfortunately handle our patients much, we always refuse obstetrical calls, as in two instances a puerperal fever in our opinion was the result. Matter from an erysipelas patient will communicate disease; we have reason to believe that we took the disease in this way ourselves, and we have seen others similarly infected."

In the report from Montgomery County, the following statement occurs:—

"Erysipelas prevailed in February and March in the upper portions of Montgomery, in the neighbourhood of Berks and Bucks counties. In Kulpsville, it assumed a malignant character, and was spread by means of the attendants upon those already affected. Erysipelas prevailed also in Harleysville, along the pike, and in the neighbourhood of Tylersport; also, about the line of Lexington. This disease seemed first to attack the throat, and afterwards the surface of the body. In females, it was especially liable to attack the peritoneum, and one or other of the serous tissues in the male. When the peritoneum became affected, there was much hiccough, from the disease extending to the diaphragm. In some cases, symptoms of arachnitis, followed by coma, presented themselves. The disease in many cases proved fatal. The treatment consisted in the administration of tonics, calomel, nitrate of silver, and the usual remedial agents.

"Many cases of puerperal fever occurred during the prevalence of erysipelas, and, so far as the committee (by whom the report was drawn up) have been able to gather from the reports they have received, these cases of puerperal fever were confined chiefly to the same localities as the former disease. Many of the cases terminated fatally."

We find repeated reference made in the reports contained in the present volume of *Transactions* to the diminished prevalence, generally speaking, of periodic fevers; to the increased frequency of typhoid fever throughout a large portion of the State; to the less sthenic character of diseases generally, and their tendency to assume at an early period a low form, precluding active depletion, and requiring an early resort to tonics and stimulants.

The following facts, presented in the report from Mifflin County, in reference to the somewhat regular cycles in which what are termed paludal fevers have prevailed in the valley of the Juniata, are particularly interesting. They are not very easily explained by the popular doctrine of a specific miasma:—

"Lewistown, the seat of justice for Mifflin County, stands upon the lime-

stone rock No. 6, and the axis, including portions of 7 and 8, is well developed on the ridge north-west of the town.

"This flourishing brough, with its 3000 inhabitants, is beautifully situated at the confluence of the Kishacoquillas Creek with the Juniata, about one mile north of the grand entrance to the narrows, from which the river pursues its winding way through the defiles of the Blacklog and Shade mountains to the adjoining county of Juniata. Since the commencement of the public improvements, Lewistown has risen rapidly to wealth and importance, and enjoys from its favourable position for trade and manufactures advantages surpassed by few inland towns in our State.

"A remittent fever of a congestive character, which prevailed as an epidemic in 1823, and was particularly rife amongst the labourers on the turnpike, and afterwards in 1828, and for several successive seasons—during the construction of the Pennsylvania Canal—gave to this place and vicinity a character for unhealthiness which, all things considered, it did not perhaps deserve. The mortality attending this epidemic was more the result of previous habits of intemperance and a total disregard of all dietetic regimen than any unusual malignity in the nature of the disease. Three-fourths of the cases were confined either to strangers unaccustomed to the atmosphere of the river, or to emigrant Irish but lately landed upon our shores."

"In 1850, and the six or seven seasons previous, a fever of a bilious remitting type prevailed in the north-eastern part of Lewistown, being the extremity most remote from the river. At the close of the season, for several years, as the weather became colder, the fever assumed the true typhoid type, and a number sunk under it with hemorrhage from the bowels. It was evidently kept up through the winter months by contagion, and is another proof of the conversion of malarial remittent, through the intervention of secondary causes, into the enteric or typhoid type.

"The fever was traced to a pond of putrescent water, connected with a brick-yard, immediately in the rear of the infected district, from which it took the name of the frog-pond fever. In 1850, when the fever was at its height, the pond was filled up, and the disease extinguished.

"From all that has preceded it is evident, then, that to the river we are principally to ascribe a class of diseases from which the other valleys are comparatively exempt. And it appears, from the earliest medical records of our county, that the recurrence of fever upon the Juniata has been marked by periods of great regularity, having a cycle of about twenty years. The first authentic account we have of its prevalence was about the year 1782-3, but of which no particular details have been left to us. It again prevailed to a great extent in the years 1801, '2, '3, and '4. From this time it nearly disappeared until the years 1823-4, when it again visited our waters, having prevailed several seasons previously on the lower parts of the Juniata and on the Susquehanna beyond.

"From this period remittents and intermittents appeared annually, to a more or less extent, on some parts of the stream, until 1851, when they left us altogether, and have not yet reappeared. Our physicians, however, agree that in 1842, and for several successive seasons, there was an unusual accession to the number of cases, so that we have had three regular cycles of twenty years, with a fourth not so distinctly marked, yet showing a tendency to conform to the usual periodic time. At the cycles of 1803 and 1823, and very probably that of 1783, the epidemic did not prevail at the same seasons upon the different sections of the Juniata, but in two instances, at least, we know that it commenced on the waters of the Chesapeake, with an upward progress along the shores of the Susquehanna and its tributaries, not reaching until the third or fourth season its ultimate boundaries upon the extreme branches of pure cold water, where the disease died out as it were for lack of the pabulum which kept it alive.

"In 1804, it reached no further than the southern limits of Huntingdon County. At every succeeding period, its upward boundary was much extended—a fact of easy explanation when we take into view the rapid progress of improvement upon our streams, the clearing out of the lands, giving access

to the sun, and a higher temperature to the waters; but above all to the noxious exhalations arising from the stagnant ponds connected with the mills, forges, and furnaces everywhere scattered over the streams. Another remarkable feature attending our febrile diseases of late years is their tendency to visceral congestion, and a gradual change of diathesis, from the acute inflammatory, which prevailed forty years ago, to the asthenic or typhoid type, which they have been imperceptibly assuming for many years past; so that the active depletion, the full vomiting by tartaric antimony, and the large doses of drastic purgatives, which were once prescribed with the best results, are now much more rarely and cautiously resorted to. Prostration of the powers of life is more readily induced than formerly, and the system sooner requires the support of tonics and stimulants, with a more generous regimen."

We had marked several passages in the reports contained in the present volume of the *Transactions* of our State Society, to lay before our readers, as presenting highly interesting observations in reference to the typhoid fever which appears to have prevailed during the past year in the interior of the State to some considerable extent; but we have already exceeded the limits within which we had hoped to confine this notice of the volume before us. For this reason, we must also omit the summary we had prepared of the experience of physicians in different sections of the State in reference to the most effective treatment of dysentery, a disease which has been of late unusually prevalent in many of the counties.

We extract the following observations on the treatment of scarlatina from the report of the Medical Society of Berks County, as deserving of attention. The free use of diluted hydrochloric acid we have ourselves prescribed in that disease, and from our as yet limited experience of its effects, we feel persuaded that it is a remedy deserving of a more extended trial. With Dr. Heister, however, we are far from believing that it will prove a specific, "to be relied on to the exclusion of other rational means when indicated."

As regards the city of Reading, Dr. J. P. Heister remarks in regard to scarlet fever as follows:—

"In my practice, it was almost exclusively confined to children, and the most prominent prodromic symptom was vomiting. After the eruption was out, and the disease well formed, the most marked symptom was delirium, which lasted four or five days, or, I should rather say nights; for, most commonly, the patient was quite rational and tranquil during the day, but very delirious at night. I find, by reference to my record, that I have treated seventy-two cases through their whole course, out of which three proved fatal. I saw three other fatal cases, all in the sequelous stage. One of these died from hemorrhage of the bowels, one from swelled glands before suppuration was established, and one (seen in consultation a few hours before his demise) from anasarca. Of the seventy-two cases treated from the first setting in of the disease, seven became anasarcaous, and none had suppuration of the glands. None of the anasarcaous cases died. I saw three or four other cases of anasarca in consultation, all of which recovered except the one above referred to.

"The whole seventy-two cases were treated by laxatives when required, which was usually at the commencement; and the unrestrained use of hydrochloric acid, diluted with water and sweetened; excepting one case in which bronchitic symptoms were prominent, and antimonials were used. From fifteen to twenty drops of the acid were directed to be put in a half pint of water, at the temperature of the chamber, well sweetened with white sugar, and allowed *ad libitum*. It was commonly very grateful to the patient, and taken with decided pleasure. No other general treatment was resorted to from first to last. From a good deal of previous experience in the treatment of this fatal disease, I was seriously impressed with the evil of the *ninia diligenter medicina*, and at first resorted to the use of hydrochloric acid rather as an abatement of this evil than from any confidence in its virtues. Sceptical as I am, however, in medicine, I

am, from my late experience, forced to attribute very considerable efficacy to it in the treatment of scarlatina. I by no means, however, consider it a specific to be relied on to the exclusion of other rational means when indicated. I think its salutary effect upon the local inflammation of the throat, when the acid is put in use at an early stage of the disease, not among the least of its benefits. It appears to me, I had fewer anginose cases to contend with than some of my brethren who did not use the acid; I certainly had fewer than I have before had in the same number of cases treated in the same period of time. I deemed it necessary to bleed in a single case, and that proved fatal, not because bleeding was inappropriate, as it for a while controlled the violence of the symptoms, but because the case was complicated with severe convulsions.

"Externally, to the throat, I used counter-irritants, preceded by leeching, where the local symptoms threatened to be violent. I found oil of turpentine and olive oil, mixed in equal parts, or in the proportion of two-thirds of olive oil with one of turpentine, applied every three or four hours, and the evaporation prevented by the application of a strip of flannel, a convenient means of attaining my object. The sloughy ulcers of the throat, when met with, which was seldom, I treated satisfactorily by the application of nitrate of silver in solution, in the proportion of two scruples to the ounce of water, applied once a day. I found that this application can be much more effectually made by means of a nicely trimmed piece of sponge, securely attached to the end of a whalebone handle, than by a camel's-hair brush. I even prefer a swab, made by rolling a narrow strip of muslin on the end of a stick, to a brush. A very important indication in young children is to keep the nostrils pervious, in order that the due aeration of the blood in the lungs may not be prevented. This is done very effectually by throwing freely into those passages, by means of a small sponge, warm sage tea, slightly acidulated with vinegar, and sweetened. Young children, too, as well as older ones, who have become exhausted by the disease, require the tough mucus, which obstructs the larynx, to be removed from time to time. This, also, is best done by a sponge, and the tea prepared as above described. Cool or tepid sponging of the surface was resorted to, with its usual tranquillizing effect. My cases of anasarca were treated principally with jalap and cream of tartar."

Dr. McDonough, in the same report, speaking of an epidemic of scarlatina which occurred at Bernville, and its vicinity, says that almost every form of the disease, from the mildest to the most malignant, was observed.

"In one family, in particular, every member, consisting of seven children, were attacked; the two oldest of whom, a boy of eighteen years of age, and a girl of sixteen, were peculiarly affected. This was in the latter part of October and the commencement of November (1851). The inflammation of the throat imparted its virulence to the bronchi, and an alarming condition of the respiratory organs soon became apparent. At this stage I was called in. The voice I found reduced to a low whisper. The tonsils and parts adjacent by no means tumefied, but ragged, and here and there ulcerated, and emitting a very fetid smell. On examining with the stethoscope, I ought to compare the thorax to a kettle with something boiling in it. The poor children could not lie down, lest suffocation should ensue. The cough was severe and dry. I at once gave them myrrh, strongly acidulated with acid. muriatic., mixed with sage tea as a gargle, and inwardly, syrup. sicc. comp., and strong vinegar, at 1 oz., to which was added half an ounce of acetated tincture of opium: dose, a spoonful morning and night. On the second visit, on the third day, I found my patients much improved. The expectoration was free and copious, the digestion much better, and they had slept soundly through the night, though in a semi-erect posture. I subsequently added a solution of sulph. quinic, and when, from personal sickness, I was obliged to discontinue my visits, their appetite was wonderfully increased."

The chairman of the committee by whom the report, from which the above is taken, was drawn up (Dr. Stewart, of Reading), remarks, in respect to the treatment of scarlet fever, that

"Regarding it as one of the family of diseases for which there is no ascertained specific, all he has aimed at has been to take care of the throat and attend to other local symptoms as they presented themselves, and to keep the system in proper order. With this view, in ordinary cases, he has for the last four years prescribed nothing beyond castor-oil to keep the bowels open, and ordered once or twice a day, during its continuance, a simplicial to the throat; and he can truthfully say that within that period he has not had a single case which ended in sore-throat. Even in cases in which this was threatened, the mustard at once prevented it."

Dr. Beaver, of Reading, in the same report, remarks as follows:—

"Dysentery had scarcely subsided when scarlatina followed. This was more general in its attacks than the former. It was mostly of the mild or simple variety, and in one case (excepting two) of the malignant form. Like dysentery, it seemed to commence in town, and afterwards spread to the surrounding country. Up to January last (1852), I treated some forty-eight cases, a great majority of which were in town; six of the above proved fatal; three of these deaths took place in adults, and the rest in children under thirteen years of age. My treatment was plain and simple, and pretty uniform; it chiefly consisted in the exhibition of a mild emetic or cathartic in the outset, as the case indicated, followed by the very free use of the hydrochloric acid given in water, *ad libitum*; always making it a point to give considerable, that the system might be affected or influenced by it. Externally, I never omitted an application to the throat, on flannel, of equal parts of powdered cubeb and black pepper, mixed with lard; a portion of which being applied was removed and then repeated every four or six hours. Not only in this disease, but wherever an inflammation of the throat and neighbouring parts exists, have I applied this external application, and know of no other that has so often produced relief and benefit to the patient."

In the same report, Dr. Wanner, of Kutztown, informs us that, besides the usual diseases of the several seasons, his neighbourhood was visited during the past year, 1851, with an epidemic of scarlatina.

"Most of the cases were of the anginose form. The treatment instituted in the twenty-four first cases consisted principally in an emetic and purge at the onset, and diaphoretics afterwards, in greasing with *speck* (the fat of bacon) the whole surface of the body; of this number I lost two. In forty cases following, the same plan of treatment was pursued, but in place of the milder diaphoretics, a prescription like the following was employed: R. Hydrochlor. ammoniac 5*ij*; magnesia sulph. 5*v*; ant. et pot. tart. gr. *j*; syrup. acid. citrici 5*ss*; aquæ 5*iv*. M. Of this solution, I gave to a child three or four years old, one teaspoonful every three hours. Of this latter number of cases I lost none. I would add, that the volatile liniment has in all the cases been used externally to the throat."

Annexed to the report from Berks County is a communication from Dr. William Gries, of Reading, on what he terms the "catarrho-nervous fever." Although differing somewhat from the views laid down by the writer in relation to the pathology of the disease of which he treats—without the time, however, to discuss at present the question—we, nevertheless, consider the paper, inasmuch as it presents the practical observations of a highly intelligent and clear-sighted physician, an interesting one. We most heartily coincide in the sentiments so forcibly expressed by Dr. Gries, in the conclusion of his essay on the defects in our present system of medical education.

The report on variola and vaccination is highly important one; we should desire to see it brought to the notice of every practitioner—of every person, indeed, in our State; and hope that the editors of our medical journals will find sufficient space to publish it, or at least that portion of it which comprises a synopsis of the evidence relied upon by the committee for the estab-

blishment of the value and certainty of vaccination as a preventive of small-pox.

We have not yet alluded to the very sensible and manly address of Dr. Charles Innes, of Northampton County, delivered as President of the Society, at the opening of its session of 1852. This has not arisen from any disrespect to its author, nor from the want of a due appreciation of the merits of the address itself, but from our having occupied as much space as can be conveniently appropriated for a notice of these *Transactions*, in our desire—but imperfectly carried out, it is true—to present to the medical community at large some idea of the fruits of the organization of the profession in Pennsylvania, so far as that organization has been effected; in the hope that our brethren in those portions of the State where, as yet, no such organization exists, may be induced, by associated efforts, to aid in the praiseworthy effort already made to increase the general stock of medical knowledge, by rendering more perfect our acquaintance with the true nature of the epidemic and endemic diseases that prevail in the different sections of the State, and, at the same time, more certain and uniform, the means adopted for their prevention, mitigation, or removal. We have, consequently, confined our very imperfect analysis exclusively to such portions of the volume as comprise observations more immediately bearing upon the etiology, the pathology, or the therapeutics of disease.

D. F. C.

ART. XX.—*Sketches of Brazil; including New Views on Tropical and European Fever; with Remarks on a Premature Decay of the System incident to Europeans on their Return from Hot Climates.* By ROBERT DUNDAS, M. D., Physician to the Northern Hospital, Liverpool; formerly Acting Surgeon to H. M. 6th Regiment; and for twenty-three years Medical Superintendent of the British Hospital, Bahia. London, 1852: 12mo. pp. 449.

THE author of these sketches is evidently a close observer and independent thinker. Many of the medical opinions advanced by him are in opposition to those currently received by the profession, and sanctioned by some of the greatest names in ancient and modern medicine; they profess, however, to have been the result of a close study of diseases—their causes and character—in different climates and localities; and, as he has presented the chief evidence on which his opinions are based, he affords his readers an opportunity of testing their truth.

The observations contained in the present publication were originally delivered in a series of lectures at the Liverpool Northern Hospital in the beginning of the present year; those lectures are here published without any material alteration.

In the introductory remarks, Dr. Dundas attacks the commonly received opinion of the specific difference between the typhus, the typhoid, and the relapsing, or, as he would denominate it, the remitting fever.

"The greater number of the distinctions, he argues, in the symptoms attempted to be established between these fevers, are obviously either trivial or visionary—distinctions without a difference; and, however broadly they may be defined in the closet, I have never yet met with a physician who could con-

clusively verify them at the bedside of the patient, although I have witnessed these diseases in this country, in Ireland, and in different quarters of the continent.

"As regards the character of the eruption, so constantly insisted on, nothing, I believe, can be more fallacious; for I have myself more than once witnessed every variety of eruption, pretty distinctly marked, in the same patient, at one period or another of his disease, and practitioners who have seen much of tropical fevers must have observed the occasional appearance of all the eruptions commonly deemed pathognomonic of special forms of European fever. Dr. Wragg, in a paper published in the *Charleston Medical Journal and Review* for March, 1851, on an epidemic of 'Breakbone Fever' in Charleston, in the summer and autumn of 1850, describes the eruption attending the disease as 'not at all peculiar in its character, but varying almost infinitely.' It resembled, in various cases, scarlet fever, rubella, impetigo, scarlet rash; in some cases the 'skin was intensely red, and raised in large and numerous welts'; 'in some cases it was erysipelatous; in others, petechial; in others, like common prickly heat; and, in a few, papular.' So vague, indeed, is the value of the eruptions as a diagnostic sign in fever, though so strongly insisted on by authors, that, even at the present hour, we are unable absolutely to diagnose certain forms of eruption from the ordinary appearance of flea bites.

"Moreover, we know that, independently of the English, many of the French, and the most distinguished German observers, whose claim to accuracy and talent few will contest, have not recognized these varieties of fever as distinct diseases. No practical physician, indeed, will admit that symptoms alone are sufficient to justify a distinct classification of fevers; and neither in the symptoms nor in the results of the so called typhus, typhoid, and relapsing fevers do we find a wider range than is constantly observed in other diseases, and is rationally accounted for by individual peculiarities, difference of season and climate, modes of living, and the sanitary condition of the population attacked, with various other moral and physical influences. In like manner, also, we can rationally explain the difference of mortality in different localities and circumstances.

"Dümmler, in his account of the Silesian epidemic of 1847, describes the fever as observing a rhythm at equal intervals like an ordinary ague, and the sequela were not very dissimilar. That it was identical with the Scotch epidemic of 1843 (and others observed at different periods in Ireland and Scotland), all admit; yet, this Silesian fever presented abundant crops of rubeculous and petechial eruptions, while the Scotch epidemic was characterized by their absence.

"Again, the prevalence of several distinct epidemics in the same locality and at the same epoch is not only contrary to general experience, but opposed to our knowledge, as far as it goes, of the laws by which epidemics are governed in all quarters of the world; nor does the term 'intercurrent' by any means satisfactorily meet the difficulty.

"As distinctive of typhus, Dr. W. Jenner, in his valuable work on fever, observes that he has never seen a relapse; a proposition, I apprehend, opposed to the general experience of the profession. He also prominently insists on the shorter duration of the fatal cases of typhus, unaccompanied by any definite anatomical signs, compared with the fatal cases of typhoid fever, in which local lesions are commonly detected, as a proof of the non-identity of the specific causes of the two fevers. Now, I apprehend that this fact admits of another and more satisfactory solution by reference to the intermittent and remittent fevers of hot climates, as well as numerous diseases of our own latitudes. In the more rapid and fatal form of these maladies, no well-defined local disorganization will be found after death; while, if the disease be prolonged, local lesions are always to be detected. In fact, Dr. Jenner himself states that, if typhus fever be prolonged beyond twenty-one days, local lesions sufficient to cause death (independently of the fever) are always present. Dr. W. Stokes, in his work on fever, a work unexcelled in any language, lays down what I believe to be the true law on this question, namely, 'that, in almost any instance of essential fever, local disease springs up at some period or other of its course.'

'and that a vast proportion of fever cases are carried off by these local affections.' He also refers to the question of time.

"I am not aware whether Dr. Wm. Stokes, or Dr. Graves, of Dublin, has visited hot climates; but the latter, in his valuable work on *Clinical Medicine*, observes that 'there is not so much difference between the diseases of Ireland and of warmer countries as has been imagined; they differ, it is true, as to their degree, but not as to their pathology.' Now, this is a remarkable statement; and supposing Dr. Graves had not himself visited tropical countries, strongly indicative of the genius of the man; for, of the absolute truth of the doctrine here so broadly laid down, there exists not the shadow of a doubt. Dr. Cormack justly remarks that, 'were this doctrine more generally appreciated, the accounts of the different fevers unfolded to us would, perhaps, present less picturesque and piquant, but certainly simpler and truer pictures of disease.' Most undoubtedly they would.

"The parallel, moreover, drawn by Dr. Graves between the Dublin and the tropical yellow fever admits of no dispute; the correspondence is complete. The same is true of the Edinburgh yellow fever, as observed by Dr. Cormack, although it differed from the Dublin fever in this, that one was of the continued, while the other was of the remittent type; there being, also, instances of intermission. But who has examined the history of the cases can for an instant question the essential identity of these fevers?

"I cannot, indeed, resist the conviction that ere long the essential identity of fever in all countries will be generally admitted by the profession; and my chief regret is, that the present advocacy of the doctrine and the opportunities which I have enjoyed of verifying its absolute truth, had not fallen to the lot of some of the eminent men whom I have named, whose talents would have done justice to its importance, and whose well-established reputation would have at once secured that attention to which questions so interesting to science and of such high practical import are so pre-eminently entitled.

"I would finally appeal to the undoubted specific influence of quinine (when opportunely and adequately administered), in controlling the fevers of tropical climates, and to its equal efficacy in controlling the fevers of this country, as has been fully proved by the cases treated at the Liverpool Northern Hospital, the Liverpool Fever Hospital, St. Thomas's Hospital, London, and elsewhere. I would ask whether, irrespective of all other evidence, the specific power exercised by proper doses of quinine over all these several forms of fever does not afford conclusive proof that in their essential nature these fevers are identical, and differ only in form and degree?"

To such as feel an interest in the important question as to the identity or specific difference of the so-called specific fevers, as well as to the advocates of the reciprocal convertibility of the several forms or types of fever, the foregoing remarks of Dr. Dundas will not prove unacceptable. To judge, however, of their full force, the extracts which the author has given from Dr. Cormack's work on the *Epidemic Fever of Edinburgh*, and Dr. Dümmler's account of the *Epidemic Typhus of Silesia*, and his references to the statements of other modern writers on the subject of fever, should be consulted.

The first lecture treats of the causes of the speedy destruction of health in European residents on their return from tropical climates.

After referring to the rare occurrence of gout among the native Brazilians, as well as among the foreign residents, notwithstanding the habits and modes of living prevalent in the higher ranks of society, and to some extent among foreigners, are such as would seem to favour the development of the disease, Dr. Dundas proceeds to account for this exemption. He maintains that the excess of certain principles in the circulating and secreting fluids of the body, which constitutes the chief predisposing, as well as the immediate or exciting cause of gout, are, in the inhabitants of tropical climates, abundantly and unceasingly eliminated by the capillary vessels of the surface.

"We must remember," he remarks, "that the cutaneous system performs infinitely more important functions in the higher than in the lower latitudes; removing through the perspiratory secretion considerable quantities of animal matter, and especially very sensible amounts of the lactic and uric acids. Topaceous deposits I have scarcely ever met with. It will thus be sufficiently apparent that the economy, under such circumstances, will be more efficiently relieved from those effete or deleterious principles which, carried into and retained in the general circulation, would constitute a real *materies morbi*, and which, by first impairing the organic nervous power, would induce disorder, or rather increase that already established in the secreting, excreting, and digestive systems. From the disturbance of the functions of these systems, plethora and increased vascular irritability would commonly result; and would give rise sooner or later, according to idiosyncrasy, constitution, and predisposition, to all the varied manifestations of tonic or anomalous gout.

"In all hot climates, the secretion from the external capillary system is incessant and profuse, while the secretion from the kidneys is in an equal ratio diminished, their functions being, in fact, vicariously and energetically performed by the skin. The urine is consequently scanty in quantity, and acrid in quality from a deficiency of its watery constituents; for example, often not more than a few ounces of high-coloured urine will be passed during the twenty-four hours, although gallons of liquid ingesta will have been taken during the same period."

Dr. Dundas has shown, from an analysis of *post-mortem* examinations of the kidneys of residents in Brazil, that, although these organs are but seldom the seat of actual disease, still they are very generally in a state of atrophy, from, as he supposes, a prolonged diminution of their function.

The condition of things above described is maintained commonly for a long series of years.

"Eventually, the individual returns to the colder latitudes of the north with his digestive powers seriously impaired, and his capillary system weakened by long-continued over-excitement. The accustomed profuse perspiration is suddenly arrested, and the renal system is as suddenly called on to resume its long dormant functions, but it is too late. The kidneys, from prolonged inaction, have suffered, if not organic disease, commonly an amount of functional change sufficient to interfere with the performance of their now all-important duties, and the most formidable consequences necessarily ensue. Gout, simple or complicated with rheumatism, appears in all its various forms, disposing to apoplexy, paralysis, disease of the genito-urinary system, functional and organic cardiac disease, or anomalous and distressing derangements of the digestive and assimilative functions, with their numberless train of diseased moral and physical sympathies."

The second lecture is devoted to a consideration of the treatment of the gouty attack by which the "break-up" of the constitution of Europeans who have returned home after a prolonged sojourn in tropical climates is ordinarily followed. The entire course of treatment, hygiene as well as therapeutic, recommended by Dr. Dundas, is judicious. We cannot spare space, however, to present even an outline of it.

The subject of the third lecture is the uncertainty of the received theories explanatory of the nature and cause of fever. The leading position of Dr. Dundas in regard to this important form of disease is thus expressed by him:—

"I am strongly convinced that much of the obscurity and uncertainty, and many of the contradictory doctrines espoused by different parties, are more to be ascribed to the imperfect views taken by the respective authors than to wilful perversion of facts or positive ignorance of the relations of various morbid actions with the series of changed functions which they involve, and of their direct or indirect connection with each other. There is a homogeneity (if the

expression be permitted) in the laws of disease, of which we are just now beginning to enjoy the first glimpses; and although it would be too much to allege that fever and inflammation are but one and the same morbid action greatly diversified, no doubt, through the influence of numerous concurrent circumstances, we nevertheless know that the theory of the day which rendered a special reason of the proximate cause of the one was deemed to be equally illustrative and explanatory of the other. The phenomena of fever, nevertheless, as specially distinguished from inflammation, are, I am disposed to believe, *essentially one and the same*; the simplest expression of that morbid action being delineated in the paroxysm of an ague—in the succession of its three stages, hot, cold, and sweating; and assuming under certain circumstances of climate, constitution, modes of living, &c. &c., the various forms of *typhus fever, plague, remittent fever, yellow fever*, and all the subordinate varieties of fever denominated *essential* by the French school."

In an examination of the doctrines of Lancisi as to the cause of paludal fevers, Dr. Dundas, by a reference to his work entitled, *De Noxiis Paludum Effluviis*, published in 1695, shows very conclusively that Lancisi is not, as is so commonly believed, the author of the modern exclusive doctrine of marsh miasma—that he nowhere teaches the idea of marsh poison being one and indivisible—of its being the exciting cause of intermittent fever by its poisonous action on the nervous or sanguiferous system—of its producing these effects in only one mode—and of its being limited to the sole generation of intermittent fever, specially and individually, as distinguished from continued fever.

We cannot follow our author in his long array of quotations from the work referred to—a work well worth the attentive perusal of all interested in the investigation of the etiology of what have been termed "miasmatic diseases."

"The authority of Lancisi," remarks Dr. D., "cannot be adduced in favour of the doctrine of a special poison; but his authority is invaluable as proving the distinct origin of every variety of fever from the agency of moisture, cold, and heat, and other atmospheric vicissitudes. In this respect, his observations become of the highest interest, and, in a great measure, corroborate the views which I have myself been led to adopt; first, as to the cause of these fevers; and second, as to the means of prevention and cure. His words on the effects produced by the effluvia are distinct and emphatic: 'Checking the perspiration by which the noxious and useless matters, which otherwise accumulate in the system, ought to be carried off from the body.' *'Unde fit, ut que nostris e corporibus perspirari dehener vel noxia, vel salutem inutilia corpuseula, magnum partem prohibentur effluere.'*"

The following is a brief summary of the leading views entertained by Lancisi on the subject of marsh poison and its effects:—

1. The poisonous emanations of marshes are not of a simple nature; but are always compound.
2. They are composed of inorganic and organic effluvia.
3. The inorganic portion can scarcely be held of itself to be a cause of disease.
4. The organic constituent—which, by the way, is almost wholly of an animal composition—conjointly with the preceding, occasions the fevers, &c.
5. These emanations are, consequently, of a very compound character, and vary very much in their physical and chemical constitution.
6. They are more especially influenced by the season of the year.
7. They produce a diversity of diseases: continued, remittent, and intermittent fevers, as well as other maladies, such as diarrhoea, &c.
8. The fevers are not always caused by marsh airs, or miasma; but they occasionally arise from a disturbed state of the elasticity of the atmosphere.

9. They are materially influenced by worms taken up by the animal body.
10. They occasionally originate where the atmosphere is in the state of highest purity.

"Lancisi never employs, throughout his work, the word '*venenum*,' or poison; and consequently he cannot be charged with the theory of marsh miasma being a specific poison. The diversity of effects, indeed, which he ascribes to its agency fully negative any such proposition. In fact, as I have already stated, Lancisi has been *quoted, not read.*"

Lecture fourth, after a review of the prevailing doctrines of malarious fevers, enters upon a discussion of the question, Is periodicity a distinctive feature in certain diseases?

Dr. D. maintains that the law of periodicity, or the disposition to remission and exacerbations at certain intervals, is not confined to intermittent fever, but will be found to apply more or less distinctly to all human diseases—to those arising in the most opposite conditions of the animal economy, and determined by morbid agents apparently the most dissimilar and opposite in their nature as well as in their results. To illustrate this, Dr. D. runs a parallel between gout and ague, both diseases being marked by distinct paroxysms and remissions; and in both, the interval between the paroxysms being attended by certain feelings of *malaise*, irritability, depression of vital power, and derangement of the secretions.

On the question, Can the supposed Poison of Ague remain dormant in the System? Dr. D. remarks as follows:—

"It has been urged, in proof that a special poison acts on the system in ague, that individuals exposed only for a brief period to the influence of marsh miasma have been seized with intermittent fever. Now, I have myself been repeatedly exposed, and have witnessed the temporary exposure of others, in robust health, and with unimpaired nervous energy, to the most concentrated emanations of marsh effluvia, in various quarters of the globe, and in *no single instance* have I ever observed such temporary exposure followed by ague. It is true that, had such persons been previously subjected to the influence of long-continued heat, cold, fatigue, intemperance, the depressing passions, &c., then, indeed, intermittent or remittent fever, rheumatism, or dysentery, or some other disease, might, and probably would, ensue, according to the constitution and predisposition of the individual. Such sudden attacks I have repeatedly witnessed to supervene on such temporary exposure in marshy localities; but I have invariably found, in these instances, that the constitutional powers of the individual had previously suffered from one or other of the depressing moral or physical agencies just alluded to. Nor, in this inquiry, must we lose sight of the fact, that a long interval may elapse between the application of the *supposed* malaria and the appearance of the intermittent, as I myself frequently witnessed in 1812, while serving as resident medical officer in the general hospital at Cadiz, the garrison of that fortress being then chiefly composed of British troops, who had been engaged in the memorable and disastrous expedition to Walcheren. Even in this country I met with a distinguished member of the English bar, who had just recovered from a severe attack of ague, supposed to have had its origin in a night journey across the Pontine marshes, under circumstances almost identical with my own. Now, this gentleman had, in the interim—that is, between autumn and spring—enjoyed his usual health; yet did his physician, one of the most eminent in England, consider that the marsh poison had lain dormant in the system during this long interval, until called into activity by a prolonged exposure, in an open carriage, to a keen easterly wind, exactly seven months after the supposed infection! The case of a lawyer, also, under precisely similar circumstances, was reported to me while in Edinburgh, with this difference only, that the period of incubation, as the phrase goes, was considerably less—about one-half—between the supposed introduction of the poison and the development of the disease. Recently, also,

Mrs. C., wife to one of the British merchants at Bahia, where she had resided for some years, and whence she had just returned, after a long sea voyage, was seized with a severe attack of ague, on the evening of her arrival in Cheltenham, months after she could by any possibility have been exposed to the influence of marsh miasma.

"Are we, then, in these and similar cases, really to believe that a poison has entered the system, and remained dormant and unchanged for months, nay, for years?

"There is something so painfully unsatisfactory in our endless succession of medical theories, that the mind, in this as in other questions, longs for a resting-place; it feels the want of a *point d'appui*, and often, I am persuaded, actuated by that feeling, sees a little, and jumps to a conclusion, less from the force of the facts and cogency of the reasoning, than through the desire of enjoying some fruits for its labour, even if it be but the mere empty satisfaction of an hypothesis."

The residue of this lecture is devoted to a review of the conflicting opinions of medical authorities as to the source, nature, characteristic progress, and effects of the alleged marsh poison.

In the fifth lecture, Dr. Dundas presents the evidence derived from his personal experience in proof of the position that marsh miasma and malaria are not the efficient cause of intermittent and remittent fever.

He commences with a description of Bahia, showing that in that city we have accumulated, in almost unexampled abundance, all those physical conditions which are deemed by physicians to constitute the elements essential for the generation of the most deadly scourges of humanity—epidemic and endemic diseases. The author further adds that, within the last twenty years, he has witnessed the city exposed, on three several occasions, to the combined horrors of siege and famine, with all their revolting continuencies.

"Yet," he adds, "notwithstanding this appalling combination of physical, moral, and social evils, universally admitted as the chief agents in producing the most extensive and fatal diseases, Bahia continued, and can, moreover, up to the present hour, boast the happy privilege of having escaped, since the period of its foundation, from every species of endemic or epidemic malady—*yellow fever, cholera, influenza, typhus, and dysentery.*"

Dr. D. remarks that the Brazilian and Portuguese merchants and shopkeepers, who inhabit the lower division of the city, and who rarely quit their dwellings, enjoy comparative immunity from intermittent fever, while those whose duties frequently lead them from the lower to the upper city often suffer from this disease, as well as from continued fever, without other apparent cause than the sudden transition from the warm, but equal temperature of the lower city, to a strong, cool, and humid sea-breeze, which they encounter while bathed in perspiration and exhausted by the labour of ascending a considerable eminence.

The singular salubrity of Bahia Dr. D. attributes to the equality of atmospheric temperature; to the serene, beautiful, and dewless nights; the healthy organization of the native Brazilian, his simple and abstemious diet, the absence of abject poverty, and the absence of those moral excitements resulting from the struggles and vicissitudes of European life.

The remainder of the lecture is devoted to a statement of facts adverse to the miasmatic origin of intermitting fever; the frequent occurrence of that disease in situations removed from all paludal and other supposed sources of miasma; and the occurrence in the same family, simultaneously, of ague, inflammatory fever, and typhus. From a careful investigation of the circumstances under which intermitting fever prevailed in Bahia and its vicinity, Dr.

D. was led to believe that it was probably attributable to exposure to moist sea-breezes. Under this supposition, adequate prophylactic measures were adopted in hospital and private practice, with the most favourable results.

The whole of this lecture is deserving of an attentive perusal. We regret that it is not within our power to present a more extended abstract of the leading facts embraced in it.

The sixth lecture commences with a description of Bomfim, a suburb adjoining the city of Bahia to the northward, and situated in the midst of a morass. It is partially subject to the influence of the tide, and consequently exposed to those deleterious effects which are believed to result from the intermixture of salt and fresh water, together with immense quantities of vegetable and animal matter, exuviae, &c., constantly acted on by the powerful influence of a tropical sun. Notwithstanding, we have here all the elements commonly deemed necessary for affording to marsh effluvia their most concentrated and deadly degree of intensity; yet Dr. D. assures us that the suburb of Bomfim enjoys the reputation of being, and is in reality, at certain seasons, one of the most healthy districts in Brazil. At that season of the year, when the sun, nearly vertical, exerts its greatest power, it is thronged with visitors, who pass a great portion of the night in the open air. Yet, during this season, a case of intermittent fever is almost unknown; while the towns situated on the sea-coast, at some miles to the northward and eastward—that is, to windward—of the hills bounding the Bomfim marsh, and consequently excluded from its influence, but exposed to the full sweep of the humid sea-breeze from the southern Atlantic, are notoriously subject toague *at all seasons* of the year.

"The history of Bomfim, during the rainy or winter season, affords the strongest additional evidence," remarks Dr. D., "in proof of the correctness of the explanation here offered. The wet season commonly sets in about the beginning of April, with a sudden change in the monsoon, when atmospheric vicissitudes—considerable for Bahia—often ensue; the rain descends in torrents, and converts the Bomfim marsh into one entire sheet of water; and, above all, the change of wind from about N.N.E. to S.S.E. at once deprives Bomfim of the protection of that range of hills already so often alluded to, and exposes it to the full force of a powerful monsoon, direct from the Southern Ocean. Under such circumstances, agreeably with the received doctrine, all noxious exhalations must be effectually arrested; yet what, in reality, do we now find to occur? We find that Bomfim, having been almost perfectly free from intermittent fever during the summer months, when it *ought* to have been absolutely uninhabitable, now becomes subject to that disease. How, then, are we to explain an anomaly so striking, and so inconsistent with the ordinary doctrine regarding the origin or production ofague? The following, I apprehend, will afford the correct solution:—

"In addition to the large amount of evaporation from such an extensive expanse of water, the powerful south-east monsoon, loaded with moisture, but necessarily free from any miasmatical contamination, now sweeps in, without the slightest impediment, direct from the Atlantic, and Bomfim is thus placed under precisely similar circumstances to the towns on the sea-coast above alluded to; and the results, as regards the health of the inhabitants, are precisely similar, and continue so until the change of the monsoon to the north-east, in October, again restores Bomfim its former hilly screen, and, at the same time, its wonted salubrity. The few permanent residents, chiefly of the lower classes, who continue to inhabit this district throughout the year, present the ordinary characteristics, though not strongly developed, of the dwellers in low and humid localities.

"It is especially worthy of remark, that, along the summit of the semicircle of hills, already described as bounding the marsh to windward, there are numerous habitations, entirely unsheltered, and constantly swept by a powerful sea-breeze direct from the ocean. All these, without exception, are greatly

subject toague at *all* seasons of the year; while, as already stated, the houses placed below, in the centre of the swamp, are only affected at certain seasons, these seasons being precisely those during which the production of marsh exhalations must be deemed entirely suspended."

After presenting many other facts and considerations, which Dr. D. believes to be altogether inconsistent with the idea of a specific morbific exhalation from marshes or other situations in which large masses of vegetable matter are undergoing decomposition, he proceeds to consider the unity of febrile diseases. These he groups into three distinct classes, namely, fever without local action, fever with local action, and fever with a special action, and arising from a specific cause.

"In all the three classes," Dr. D. remarks, "the common feature of family resemblance is portrayed in the initial symptoms—the nervous depression of longer or shorter duration, the cold shivering and anserine skin, followed by the glowering heat and general perspiration.

"It is, indeed, more than probable that fever is the expression of a type of disease essentially one and uniform, but admitting of an almost endless variety of forms, the simplest being displayed in the single paroxysm of an ague. It is furthermore obvious, on the most cursory observation, that the description of fever as a disease has, in the generality of instances, been drawn for certain localities, and not from the whole group of febrile diseases, as witnessed in different parts of the world. The typhus fever of this country is superseded by the bilious remittent and intermittent in southern climates, by the plague in the Levant, and by the yellow fever within the tropics. Each of these maladies, under the special influence of climate, temperament, different modes of living, and numerous other agencies, affects certain peculiarities in its progress; but they are all distinctly impressed by the phenomena universally characteristic of fever as a genus of disease in every clime.

"Climate, indeed, and its varieties, whether permanent or temporary, always modify the animal economy, and induce, if I may be allowed the expression, an epidemic or endemic state of the human constitution exposed to its influence. This state will vary as the atmosphere may be cold or hot, dry or humid, pure or impure, variable or equable, stagnant or the reverse; the geological character of the country or district, also, and the numerous moral and physical conditions already alluded to, powerfully contribute towards the formation of this endemic constitution in man. Moreover, the history of epidemic and endemic diseases almost universally proves that, prior to their invasion, the country or district has been subjected, for a period more or less prolonged, to some well-marked deviation from the ordinary constitution of the inhabitants generally; so that the slightest disturbing cause, moral or physical, is followed by disease, the characteristics of which are determined as much by the constitution of the individual, as by the constitution of the atmosphere.

"An extensive acquaintance with the fevers of different regions of the globe has gradually established in my mind a conviction of their essential identity. And this has been confirmed since my return to Europe, in 1842, by my observation of the typhus fever of this country and of France, which, in its more intense form, differs rather in degree than in essence, from the remittent of hot climates, or the yellow fever, as I observed it during the epidemic of 1812-13 at Cadiz, while serving as resident medical officer at the general hospital for the reception of the sick of the army of Andalusia. In each, you have commonly the same tendency to remission, and there is no one symptom, even to the yellow skin and black vomit, which you will not find occasionally present in each of the three diseases, neither is there any one organ or system so constantly affected, nor any lesion so constantly detected on *post-mortem* examination as to characterize the several maladies. There is no one morbid result, in fact, in any of these forms, that you will not occasionally find in all."

"The remittent, or bilious remittent, of southern climates, I need scarcely observe, is simply a more intense form of intermittent, and differs only in severity of the symptoms, the less perfect remissions (intermission really ap-

plies to neither), and the greater disturbance of the organic functions. They are to be treated on the same principles, and are controlled, fortunately, by the same remedies."

The seventh lecture treats of the arrest of intermittent, remittent, and typhus fever by large and repeated doses of quinine. Dr. D. denies that the action of quinine is that of a tonic in the ordinary sense of the term; he maintains that its action is obviously on the nervous system, whose functions it favourably and rapidly modifies when they are depressed or exhausted by any of the numerous moral and physical agencies enumerated as the causes of fever, and thus it restores to the organic nervous system its normal influence over the animal fluids and the vital phenomena.

In a consideration of the exciting cause of fever, Dr. D. lays down the doctrine that nervous and vascular exhaustion being induced by excessive heat, laborious exertion, the depressing passions, &c., frequently accompanied by intemperance and other excesses, exposure to cold, damp atmosphere, or the sudden diminution of the temperature of the body, will then be sufficient to induce fever of the most intense grade.

"If we examine," Dr. D. remarks, "those instances which are especially put forward in proof of the action of a morbid poison on the system, we shall commonly find that they refer to soldiers and sailors engaged, in hot climates, on detached or fatiguing duty, in dockyards, on watering expeditions, &c., and exposed to the intense rays of the sun, rendered doubly oppressive by reflection from the sides of the hills, or in the deep valleys, where such operations are most usually carried on. The cutaneous system (both in its nervous and its vascular elements) is brought into a state of the highest excitement by excessive heat and laborious exertion, frequently accompanied by intemperance and other excesses. Extreme exhaustion of the nervous system necessarily succeeds, and is followed by a collapse. Night comes on, with a positive difference in temperature, often more than thirty degrees, between the mid-day heat and the earth's surface towards sunrise. The nervous energies, animal and organic, being already profoundly depressed, and the faculty of evolving heat proportionately diminished, the men fall asleep, not only deprived of their ordinary protection, but with their light tropical clothing drenched in the chilly night-dew, and exposed to a brisk land or sea-breeze, to awake in all the horrors of ardent fever. A swamp, a morass, a fen, a something unknown (for, even advanced as we are in the knowledge of the laws of nature, there exists in the human mind an inherent tendency to look to some secret, hidden, and unrevealed influence), is at once accused as the sole agent, and, thus satisfied, all inquiry ceases. Now, this train of argument—inferring a pre-existing cause from certain effects—may be perfectly legitimate; but the cycle of logic is not equally correct, assuming that cause to be an unknown quality or essence, while it is matter of observation to every one that similar effects result from causes *known* and *patent* to all.

"For example, under the foregoing circumstances, the influence of cold and exhaustion, positive as well as relative, on the sentient extremities of the nerves, and the rapid depletion of the vascular system by profuse tropical perspiration, seem to be altogether forgotten. In such cases, the depressed or exhausted energies of the cutaneous nerves will lower, and occasionally even paralyze the capillary circulation, and thus deprive the nervous centres of the vivifying influence of vitalized blood; for the activity or diminution of the circulation depends less on an action in the heart than on an action in the capillaries. The energy of the peripheries of the incident nerves being thus depressed, the respiratory movements lessened in proportion to the diminished activity of the organic nervous functions, imperfect arterialization of the blood, with deficient circulation in the pulmonary and systemic capillaries, and defective depuration, necessarily ensue; and are followed by an arrest of the evolution of animal heat, and of the normal reaction between the blood and tissues—results highly deleterious at all times, but in some constitutions, and in certain states

of the system, as fatal as the most deadly poison, and often with almost equal rapidity. Surely, under such circumstances, we need not evoke the phantom Malaria, in order satisfactorily to account for the supervention of fever, or any other serious malady.

“ *Nec Deus intersit, nisi digni vindice nodus.*”

“ Neither has the influence of an occasionally large and sudden subtraction of the saline constituents directly from the serum of the blood, incident to over-exertion and exposure to the sun in hot climates, been hitherto duly estimated by physicians. Physiologists teach, and pathology confirms the fact that those elements are essential to the preservation of the circulating fluid from decomposition, for the black and dissolved condition of the blood, observed in the more rapid and fatal forms of tropical fever, is found almost invariably allied with great diminution or entire loss of its saline constituents. And this, I apprehend, is due, not to the exhaustion of these elements by the violence of the fever, but chiefly to their removal, which precedes the invasion of the disease and greatly influences its course.

“ Moreover, you must all be cognizant, from the experiments of Hunter and others, of the immediate and decided impression which the sudden loss of even a few ounces of blood exercises on the general system, and, through it, on the constitution of the entire mass of the blood itself. So, likewise, will the exhaustion of the nervous power, and the sudden and rapid depletion of the animal fluids by the rapid and profuse perspiration incident to all laborious exertion in hot climates, often profoundly and immediately modify the whole animal economy. Thus, indeed, can we more satisfactorily explain the occurrence, from simple exposure to intense solar heat, of fevers similar in character to the most pernicious fevers of marshy districts, as observed by Dr. Murray; and most medical officers who have served in hot climates must have witnessed, as I have, a perfectly healthy man *walked* into a pernicious fever, ay, and quickly, when the atmosphere has been humid, hot, and sultry.

“ In offering a solution of the above and analogous facts, we are much aided by the evidence of Sanctorius and others, who have shown that, independently of its direct depressing influence on the vital powers, a drenching perspiration (never absent under the above circumstances) will arrest, or seriously interrupt, the elimination of those digested or effete elements which are alone removed by insensible exhalation, and whose retention in the system is ever attended by the most formidable results.

“ Moreover, the immediate influence of the organic nerves, in modifying the constitution of the blood, is now placed, by direct experiment, observation, and experience, beyond all rational question; nor can it be doubted that such morbid changes can be suddenly impressed on the blood, through the nervous system, as to transform the healthy man of to-day into a mass of disease by to-morrow, and that, too, independently of the introduction, *ab externo*, of a single particle of any morbid agent into the system. I need scarcely allude to the daily observed fact of the influence of a serious nervous shock in morbidly modifying the animal fluids. An instructive case in point, of fatal yellow fever following amputation in consequence of accident, is given by Sir George Ballingall, and our own hospital affords numerous analogous instances; whilst Dr. Hodgkin, in his interesting and valuable observations, has shown that, in certain states of the constitution, even a slight lesion is adequate to the production of similar morbid results—a fact to which I myself can bear testimony.

“ We know, also, on the authority of Dr. Cheyne, that the most fatal cases of typhus fever will originate solely in anxiety and mental depression, without the individual having been exposed to any contaminating influence whatever; and I have repeatedly witnessed intermittent fever, of the most intractable character, originate under precisely similar circumstances, and, on one or two occasions, apparently from accidental loss of blood.

“ How, also, does it occur that careful attention to all those measures which preserve the general health, sustain the nervous power, and support the capillary circulation, will completely annihilate the virulence of marsh poison? This is clearly proved, in numerous and well-authenticated instances, on the

coast of Africa, where, by careful attention to the general health and habits of the men, the use of woollen dresses, and protection by awning from the sun during the day, and during the night from exposure to the chill and humid atmosphere, ships' companies and boats' crews have been maintained in a state of perfect health, notwithstanding a long-continued exposure to all the ordinary exciting causes of African fever. Even so lately as July, 1850, we are told, on high authority, that with regard to the alleged unhealthiness of it (the coast of Africa), so nearly have European skill, science, and care, baffled the climate, that the African station is now as healthy as the rest of our naval stations in the tropics. And we are indebted to that distinguished physician, Sir James Clarke, for the following important and analogous facts—facts quite in accordance with my own experience in different quarters of the globe, but utterly inconsistent with the notion of a *specific poison*. He says that a person may sleep with perfect safety in the centre of the Pontine marshes, by keeping his room 'well heated by a fire during the night'; and again, that 'the exemption of the central parts of a large town from these fevers (malarious) is explained by the dryness of the atmosphere, and by the comparative equality of temperature which prevails there.' How does this occur? If there be a poison, it must be taken in either by the air-passages, or in deglutition, or absorbed by the skin. 'Having his room well heated' certainly cannot prevent the access of the malarial to the skin, or to the lungs; on the contrary, from the current maintained by a fire, a greater amount of air, and consequently of the poison incorporated with it, must necessarily have access to the lungs at each act of respiration. Deglutition taking place only when we are awake, all access through this channel is necessarily denied. Can we then believe that simply warming a room causes the poison to refuse to enter the system by the skin or by the lungs, though extensively diffused through the atmosphere? or, have we not, indeed, this obvious solution to the problem: *THERE IS NO POISON?* *De non apparentibus et non existentibus cedens est ratio.* Is not the dryness of the atmosphere, as well as its warmth by means of a fire, an adequate protection against the reduced temperature of the night, and little swerving from that enjoyed during the diurnal heat? Thus are sustained, especially during sleep, the organic nervous power, and consequently the activity of the universal capillary and respiratory functions, through which the normal reactions, so essential to health, are promoted and maintained.

"Such I believe to be a true solution of the foregoing remarkable and well-established fact, and explanatory of the invasion of or the immunity from febrile diseases in all warm climates. In those countries, you will please to recollect, the capillary vessels play a much more important part than in colder latitudes, and are, consequently, more exposed to derangement from their excessive and incessant action, maintained by the permanent stimulus of high temperature. This important system, therefore, soon becomes weakened, as do also the peripheral nerves, whose energies, being exhausted or depressed from the same cause, greatly increase, if they do not indeed originate, the general mischief. Prolonged exposure to the sun, residence in humid and sultry localities, excessive fatigue, debauch, the depressing passions, exhaustion from previous disease, &c. &c., will equally depress the organic nervous power and the capillary circulation, and thus establish a morbid sensibility and susceptibility to serious modification from such limited atmospheric changes as are caused by a strong humid breeze or other equally slight physical or moral influences, which, in a different state of the economy, would be either unattended with evil, or would pass altogether unnoticed. Moreover, in hot countries, all the internal organs sympathize largely and immediately with the slightest derangement of the dermoid membrane—and I am deeply convinced that such derangements constitute the immediate exciting cause of nineteen-twentieths of the diseases (including all the fevers) to which Europeans are subject in hot and tropical climates.

"The comparative exemption from fever enjoyed by the Negro race in hot climates is another well-established fact; and its solution, I apprehend, will be found chiefly in the peculiar structure of the dermoid membrane of the black, in the abundant and often highly-offensive sebaceous or oleaginous secretion

with which the surface of the African is naturally anointed, and which preserves him against the effects of atmospheric changes. Also, the more energetic performance of the functions of the cutaneous system in the Negro, as compared with the European, renders him less subject to exhaustion in the hot and humid atmosphere of his native climate."

This perhaps unreasonably long extract from the work of Dr. D. will give our readers a very correct idea of his views in relation to the etiology of an important class of fevers—those usually considered to be the product of a miasmatic or malarious influence. These views are by no means new; they have been already broached and defended by several medical authorities of distinguished eminence. Dr. D. has presented them, however, in a very clear and concise form, and besides, has given them additional weight by the results of his own observations collected during many years' experience in many lands. They demand a careful consideration. The doctrine of a specific poison, generated during the slow decomposition of vegetable matter as a cause of fever, is fast losing ground, while the more rational opinion thus expressed by Dr. Bascome (*History of Epidemic Pestilences, &c.*, London, 1851); is attracting increased attention and additional advocates, as the etiology of endemic and epidemic diseases is more closely and systematically investigated.

"I take leave," says Dr. Bascome, "to reiterate my opinion—an opinion founded on a careful review of the foregoing history of epidemics—that all epidemic pestilences or diseases are to be accounted for on the principle of natural causes, viz., that atmospheric disturbance, consisting of variations of temperature, by hygrometric influence, atmospheric pressure, electrical tension, &c., are the exciting causes; while, on the other hand, want of light, impure air, especially from the want of ventilation, in which are included malaria and all other noxious vapours, from whatever source arising, scanty diet, and habits induced by the irregular artificial life of man, are the predisposing causes, which, by enervating and otherwise spoiling the system, render it more susceptible of external atmospheric impressions in the production of epidemic pestilence or disease."

The eighth lecture is devoted to a consideration of the causes of salubrity in Bahia, its equal temperature, its dry cool nights, its never-failing breeze, preventing a stagnant or calm state of the atmosphere, the temperate habits, orderly, cheerful, and equable disposition of the inhabitants. Dr. D. notices the recent political changes in Brazil, entailing others affecting the social and moral condition of the people of Bahia, and the influence which these changes have already exercised on the character and frequency of certain classes of diseases. He specifies the greater frequency of insanity, the increase of continued fevers, and of suppurative hepatitis, and the decrease of elephantiasis arabum. An interesting description of the latter disease as well as of the elephantiasis gracorum, which is of common occurrence at Bahia, is given.

The ninth lecture treats of the state of medical education in Bahia, and of the character and condition of the medical profession there. Having already extended our notice of the very interesting work of Dr. D. far beyond the limits we had originally intended, we cannot indulge in an examination of the topics embraced in the two concluding lectures. They, in common with those which precede, will amply repay an attentive perusal.

Appended to the volume are two papers, the one "On the Efficacy of Large and Frequent Doses of Quinine in arresting the Course of Continued Fever," originally published by the author in the *London Medical Times*, and the other "On the Arrest of Typhus Fever by Cinchonism," which appeared originally in the *London Journal of Medicine*.

From the latter we quote the following summary of the pathological and therapeutical views maintained by Dr. Dundas:—

"1. Ague and remittent fever do not originate in malaria or marsh poison.

"2. Intermittent, remittent, and continued fever are mere varieties of the same disease. The intermittent constantly merges into the remittent or continued type, and continued fever assumes still more frequently (in Brazil) the intermittent form, and all are curable by the same agents. By the same agents we can arrest them all. Could this be done if they originated in different specific poisons? or can we arrest by any power the course of a truly specific disease, as smallpox, &c.?"

"3. The notion of typhus fever being unknown in the tropics is altogether unfounded. The intermittent and continued fevers of tropical climates often run into genuine typhus.

"4. Bark duly administered will generally arrest the intermittent and remittent fever; and typhus fever being essentially the same disease, bark ought to, and will generally arrest it.

"5. Ague will occasionally resist for many days the most judicious applications of quinine—and finally yield; the same remark applies to the typhus.

"6. Quinine is more certain in its results in proportion to its early administration, but is less to be depended on with the aged.

"7. The administration of large doses of quinine in typhus, when not curative, is never followed by the slightest ill effects.

"8. As typhus is commonly the more severe form of fever, and the subjects of its attacks generally less favourably disposed, so we shall find considerable discrepancy in the several results.

"9. Typhus will occasionally resist quinine, and yield to other remedies, and the same holds good with ague; yet, who ever associates the latter disease with any other remedy than quinine? And I am firmly of opinion that the time approaches when the treatment of typhus fever, after ages of vacillation, will be established on the same sure and satisfactory basis as that on which the treatment of ague now rests."

D. F. C.

ART. XXI.—*Du Rachitis, de la Fragilité des Os, de l'Osteomalacie.* Par E. J. BEYLARD (de Philadelphie), Docteur en Médecine de la Faculté de Paris, Membre de la Société Médicale d'Observation, Membre Correspondant de la Société de Biologie. Paris, 1852: 4to. pp. 285. With 8 plates. *On Rachitis, Fragility of the Bones, and Osteomalacia.* By E. J. BEYLARD, M. D., &c. &c.

DR. BEYLARD is, as the title-page of his book declares, a Philadelphian, who has pursued his medical studies, and obtained his degree in the doctorate of medicine in Paris. The volume which he has here presented to the profession is his inaugural thesis. The opportunities which Dr. Beylard has enjoyed for investigating the interesting and obscure pathological conditions or condition of which he has treated in this volume have been very great; particularly from the fact of his connection with Professor Rousseau; as also from his having had access to the large public and private pathological cabinets of the metropolis of France. From these sources of personal information, as well as from an extensive and careful study of the literature of his subject, he has derived the materials for his essay.

The prominent object of the author, in the prosecution of his investigations, "was to establish the position that the softening of the bones which some-

times affects adults differs in no respect from that morbid condition of children which is known as *rachitis*. The symptoms, the progress, the pathological anatomy, the effects of treatment, are the same in both; or, at least, the slight differences are readily explained by reference to the peculiarities of the skeleton at the different periods of life." (p. 20.) Accordingly, these affections are rigorously compared in all their pathological features; and we presume that most of our readers will, after perusing Dr. Beylard's volume, accept the conclusions at which he himself arrives.

We would not be understood to imply that Dr. Beylard assumes any claim to originality in adopting and urging this view. On the contrary, in many parts of the volume, frequent allusion is made to both ancient and modern authors who maintain the same opinion. Indeed, he states that, "until the time of Levacher de la Fleurtrie (in 1772), no writer attempted to establish a fundamental distinction between the softening of the bones of adults and that occurring in children" (p. 15); and the opinion most commonly received at present admits, we think, their identity.

After presenting a succinct historical sketch of the disease in question, in which the varied phases of conjecture, speculation, and observation which have been at different times elicited are briefly noted, the author fairly enters upon his subject by an investigation into the *etiology* of rachitis and osteomalacia, to each of which a separate chapter is devoted.

With regard to the important question as to the transmission of these affections from parent to offspring, Dr. Beylard inclines to the actual occurrence of such hereditary descent in both cases, and produces very apposite illustrations in support of his belief. But, as he remarks, it is not easy, particularly in respect to the softening of the bones of adults, to draw a positive line of separation between the influence of an hereditary taint, and that of accidental hygienic causes in the production of the morbid condition. It is difficult, however, in view of the instances cited here and elsewhere, to deny to the former a certain degree of predisposing, if not of more active, agency.

Many writers upon rachitis and its kindred affections ascribe to the operation of the scrofulous and tubercular cachexies a very considerable share in the causation of the first named diseases. Dr. Beylard does not admit this idea, but asserts, on the contrary, that the former excludes the latter. In the Children's Hospital in Paris, it is observed that there is about one rachitic child to eighty afflicted with scrofula; and the remark made by Rokitansky is a very cogent one in favour of this antagonism; he says of rickets: "It is associated with tubercle very rarely, *considering that the deformity of the thorax, which rickets frequently occasions, brings on conditions suited to the development of that disease*." (Pathol. Anat. vol. iii. p. 174, Sydenham Soc. ed.)

Rickets affects *female* children rather more frequently than males. Thus, according to M. Guerin's observations (*Mém. sur le Rachitis*), of 346 cases of the disease, 198 were girls, and 148 boys; Dr. Beylard admits this proportion. *Osteomalacia* likewise manifests a decided preference for the softer sex according to Marjolin, in the proportion of 20 to 1 of the male; to Gaspari, of 13 to 8; to Beylard, of 36 to 11 (p. 31). Thus it will be seen that females are even considerably more commonly the subjects of osteomalacia than of rickets. This circumstance is generally attributed to the influence of *gestation* in the adult female, which is, accordingly, by most writers considered as a powerful determining cause of softening of the bones. In 36 cases of this affection collected by Dr. Beylard among women, 15 of the patients had had children; in 16 no mention is made with reference to this point;

and the remaining 5 were girls who had had no children. Some writers have gone even so far as to maintain that there is a necessary connection between pregnancy, and lactation, and softening of the bones. Thus, M. Gubler (quoted at p. 19) contends that pregnancy *commonly* produces a certain degree of this condition. His chief arguments in support of this idea are, that phosphatic earths are largely excreted in the urine of pregnant women, as in that of persons suffering from rickets or osteomalacia; that reparation of injuries to the bones of pregnant women takes place tardily and imperfectly, as in the diseased states just mentioned; and he considers "the puerperal osteophyte," found so frequently on the internal face of the cranial bones of women who have died subsequently to conception, as the result of an effort of nature to repair a softened condition of those bones, similar to the process occurring in rickets. So that, according to M. Gubler, "osteomalacia, properly so called, which most commonly follows pregnancy, is but a more advanced stage of the same rudimentary, and as it were normal, softening of the bones which has been hitherto misunderstood."

But such arguments as these, urged as conclusive and exclusive for M. Gubler's position, are worth nothing; for, first, cases of mollusities ossium are not very infrequent in which the urine exhibits no phosphatic deposits (Stanley on *Diseases of Bones*, Am. ed. p. 191; M. Curting, *Med. Chir. Trans.* vol. xx. p. 371; Mr. Solly, *Med. Chir. Trans.* vol. xxvii. p. 441); and, moreover, the presence of phosphatic earths even in large quantities in the urine is of very common occurrence when the nervous system is in a depressed condition generally or locally, owing to functional as well as organic disturbances of the cerebro-spinal or organic nervous centres, without the osseous system being at all implicated. Secondly, the instances in which union does not occur after fractures in pregnant women are exceedingly exceptional, as is evident upon reference to statistical tables of fractures. Indeed, pseudarthrosis in the female is infinitely more rare than in the male; Dr. Norris (*American Journal*, Jan. 1842), in 147 reported cases found but 18 accredited to females; and Malgaigne (*Traité des Fractures*, p. 139) never saw but one instance of false joint after fracture in a female, and in this the patient was only three years old. The same inference is deducible from the remarkable paucity of recorded examples of non-union. Walker, of Oxford, met with only 6 or 8 in about 1000 fractures in both sexes; Liston saw in his large practice only 1; Peirson only 1 in 367; at the Pennsylvania Hospital, in 946 fractures treated from 1830 to 1840, not an instance occurred; according to Mr. Lonsdale, in nearly 4000 fractures brought to the Middlesex Hospital during ten years, only 5 or 6 examples of non-union were seen. (Malgaigne.) Now, if pregnancy or lactation necessarily induced disposition even to a diminution of reparative action in the osseous system, records of non-union after fractures, and various other evidences of impaired and defective nutrition in the bones of pregnant and nursing women, would be much more common than they really are, for they are as liable to fracture as others. Finally, if "the puerperal osteophyte" is truly the expression and the result of a reparative action set up to remedy a precursory softening of the inner surface of the cranial bones, we should expect to find here and there patches of softening, at which points the reparation had not as yet commenced; but neither Rokitansky nor Ducrest makes any mention of such observations.

We take the opportunity of correcting here a mistake into which Dr. Beylard has inadvertently fallen. He speaks, at p. 19, of M. Ducrest as having discovered the existence and frequent occurrence of these osteophytic productions upon the inner surface of the crania of women who had died during or

after pregnancy. This interesting discovery was made by Rokitansky, in Vienna; and the first account of the same was published in the *Oest. Medic. Jahrbüch*, vol. xv. (Rokitansky's *Pathological Anatomy*, vol. iii. p. 208, Syden. Soc. ed.). In fact, M. Ducrest himself mentions, in his essay (*Mém. de la Soc. M. d'Observation*), that a friend had informed him that a paper upon this subject had already been published "in a German journal," which, however, he (Ducrest) had not seen.

But to return to the relation existing between the puerperal condition and osteomalacia. Dr. Beylard, as does every other writer, acknowledges that a connection of some sort does exist between them, although he does not adopt the extreme views just alluded to. He expresses himself thus:—

"The influence of gestation or of parturition is indeed manifest upon the development and progress of the disease; for, after each renewed conception, there is a very sensible aggravation; the number of repetitions, however, is but of secondary consequence, the disease showing itself as well after a second or a third confinement, as after a fifth or a sixth." (p. 32.)

This effect may be merely accidental, induced by the impoverished nutrition and the depression which pregnancy and suckling, particularly if frequently repeated, are so often seen to occasion among the poor. Or it may be, indeed, as many suppose, a consequence of the abstraction from the blood of the mother, during the period of pregnancy and lactation, of the earthy materials which are essential to the constitution of the tissues, especially the osseous, of the child. So long as a proper supply of food is furnished to the parent, her assimilative functions being in a healthy condition, these elements are derived from the blood thus formed; but, under less favourable circumstances, it is probable that those of her tissues which contain the earthy elements most abundantly, or in a form most easily acted upon, will be deprived of them to the requisite degree. This supposition receives countenance from the observations that the teeth of pregnant women frequently become carious, and even loosened in consequence of absorption of the alveolar processes and enlargement of the sockets (Hohl, *Zur Pathologie des Beckens*, p. 99; Leipzig, 1852); that pregnant women very commonly manifest a craving for earthy matters; that, if fowls be deprived of their accustomed allowance of calcareous food, the eggs which they lay are destitute of a firm shell, their bones become softened and are easily broken, and the bones of their young are soft and flexible; it was observed, moreover, in some of these experiments, that death was preceded by catarrhal diarrhoea, as is often the case in the human sufferer from mollities ossium, agreeably to the experience of Dr. Beylard (p. 49), and others (Heule, *Rationellen Pathologie*, vol. ii. p. 372).

The influence of age upon the production of rickets and osteomalacia is next inquired into. Of the three hundred and forty-six cases of rickets, adduced by M. Guérin, two hundred and nine became affected before the commencement of their fourth year; of these two hundred and nine, one hundred and seventy-eight began to suffer during their second year, and ninety-eight during their first. The majority of cases seen by Dr. Beylard occurred towards the close of the first year, or during the first six months of the second; and the frequency of their occurrence diminishes rapidly after this early age, until the period of puberty is passed, when the same disease, as Dr. Beylard thinks, again exhibits itself under the title of "osteomalacia," "rachitis adulorum," &c., its most common period being the ages of thirty and forty, after which it becomes more infrequent.

Dr. Beylard seems to have entirely overlooked an important contribution to this part of the pathology of the osseous system, by Dr. Elsässer, of Stuttgart.

This essay (*Der weiche Hinterkopf*, Stuttgart, 1843) possesses the more interest and consequence, because, although the occurrence of rickets during the first months of infantile life, and even as a congenital affection, had been long before witnessed and recorded by many writers, its frequency had, previously to Dr. Elsässer's publication, been very much underrated. This gentleman saw *forty* cases of this disease in five years, and in most of these the first symptoms manifested themselves within the *first six months after birth*, a period much earlier than that which is generally, and, by Dr. Beylard, assigned as the era of the ordinary invasion of rickets. Dr. Elsässer applies the terms "soft *occiput*, or *craniotabes*," to the particular phase of this affection as he observed it, because it attacked the cranial bones, and especially the occipital bone, producing in these changes precisely analogous to those which rickets occasions in other parts of the skeleton. When a cure was accomplished, which was the case in twenty-six of the forty cases, it was by an improvement of the general health, and by induration and consolidation of the previously softened bone, as in older persons; moreover, it not infrequently happened that other bones became involved in the disease either subsequently to its cure in its original seat, or while it was still progressing there. Like rickets and *mollities ossium*, as described by other writers, the *craniotabes* of Dr. Elsässer exhibited its dyscratic character by the evidences of deranged health and its erratic and diffused attacks on various parts of the skeleton. We cannot forbear quoting one of the conclusions which the author draws from his observations upon this disease in all periods of life; we think it may serve to explain why, at particular epochs, it should preferably affect particular portions of the osseous system. "The erratic character of rickets is a fact beyond dispute. The rule, according to which this wandering of the disease takes place, is intimately connected with the phases of development of the principal parts of the body. It usually establishes itself in whichever part the vegetative and functional endowments are in the most active and energetic state of development. In this respect, the head, thorax, and extremities may be distinguished. With the changes in the development of the viscera, there takes place a corresponding change in the development of the bones which inclose them." (See a notice of Dr. Elsässer's book, in the *British and Foreign Medical Review*, vol. xvii.)

The symptoms of the disease in the youth and in the adult are next investigated by Dr. Beylard, and carefully compared the one with the other. It is unnecessary that we should accompany him through all these details. Suffice it to say that, viewing the entire range of symptoms, we have in no other essay upon this subject met with a more full and minute account of the progress of the disease, from the first exhibition of mere functional derangement of various organs to the fully-established change of structure in the bones themselves. The author thus indicates the conclusion at which he arrives from his careful comparison of the symptoms of softening of the bones at different ages; a conclusion, let us observe, in which he is sustained by the facts adduced.

"From the comparison which we have just presented of the symptoms of softening of the bones at different ages, it is seen that most of them completely accord with each other. If there is some slight differences, if the assimilation is not greater, it is in consequence of the rarity of rickets among adults. After a while, when the number of cases recorded shall have become increased so as to permit of a more thorough investigation, we have no doubt but that these discrepancies will be entirely reconciled." (p. 98.)

And again, at p. 100, he says—

"It is evident, from the parallel which we have instituted, that there is not a single symptom belonging to one of these two affections which is not common also to the other, in a different degree, it may be; but the physiological differences of the ages are so marked that they necessarily exert an influence upon the pathological phenomena."

Dr. Beylard would have made his position still stronger in favour of the identity of the two, if he had compared in a more direct and pointed manner that portion of the skeleton upon which rickets and osteomalacia are stated to impress their most distinctive characters, and to the peculiarities of which the supporters of the diversity of the two affections most complacently refer. We allude to the *pelvis*. It is undoubtedly a striking fact that at different periods of life the same disease should affect different parts of the osseous system, differently affect the same part, and even seize upon different portions of the same bone. Thus, admitting that rickets and osteomalacia constitute a pathological unit, we find it, according to Elsässer, most commonly attacking the cranial bones in the earliest months of infantile existence; a little later manifesting itself most constantly in a series of enlargements at the sternal extremities of the ribs, in connection with other modifications of the thoracic parietes—the whole forming a group of evidences much more pathognomonic, as Dr. Beylard thinks (p. 41), than any other at this age. Still farther on in childhood, the most prominent changes seem to be the enlargement of the articular extremities of the long bones (which is rarely seen after the age of fifteen to twenty, according to Dr. Beylard), the curvature of their shafts, and the consequent shortening of these bones; this category of symptoms is so constant in childhood that, in one hundred and ninety-six cases reported by Guérin, he saw but eleven in which there was not swelling of the epiphyses and diminution in the length of the long bones. In adult life the spinal column and the pelvis are the especial sufferers, the latter having impressed upon it such alterations of form as, agreeably to many very distinguished pathologists, permit it to be recognized and distinguished as the *osteomalacia*, in contradistinction to the *rachitic* pelvis. It is upon the truth or falsity of this assumed characteristic difference, we would have had Dr. Beylard more argumentative.

He says, at page 40, in speaking of the effects of *rickets*: "The bones of the pelvis become flattened (*s'aplatisent*), and everted (*se renversent en dehors*); the diameters are shortened, the sacro-vertebral angle becomes more prominent, the ischia approach each other." We believe that the chapter on the symptomatology of rickets contains no other mention of pelvic deformity than the above. At page 52, he remarks, with reference to the influence of *osteomalacia* upon the adult pelvis: "The bones of the pelvis are spread out (*s'étalent*), the diameters are diminished, the crest of the ilium is everted (*se renverse en dehors*), and the sacro-vertebral angle is thrown forwards;" the chapter on the symptomatology of osteomalacia makes no other mention of the peculiar alteration impressed upon the shape and dimensions of the pelvis; and in that in which the symptoms of the two affections are compared, there is nothing more explicit or satisfactory upon this point. (*Vid. pp. 92-3.*)

The characteristics of the *rachitic* pelvis, as given by Kiwisch (*die Geburtskunde*, ii. Abth. s. 178), are "more or less remarkable smallness of the pelvis as a whole; diminished height of the same; compression of the pelvis in the direction of the conjugate diameter, so that the sacrum is thrown nearer than normally to the anterior wall of the pelvis, while its lateral parietes are more widely separated from each other. There is also a widening and flat-

tening of the pubic arch; an increased inclination forwards of the upper part of the sacrum, with a coincident recession of the inferior portion, whereby the straight diameter of the outlet of the cavity is relatively increased in length; a diminution of the concavity of the sacrum, of the pubes, and of the inner face of the iliac fossæ. A very common circumstance, likewise, is a more or less considerable lateral deviation of the sacrum, so that the pelvic cavity is rendered unsymmetrical." The same author gives the following as the most common form of the *osteomalacic pelvis*, p. 179: "The deep, almost excavated concavity of the iliac fossæ; the abnormal shape of the superior strait (like that of the heart on a playing-card); the unusually deep situation of the promontory of the sacrum, which is about opposite the middle of the pubic symphysis; the moderately beak-shaped projection of the anterior wall of the pelvis; the bending of the inferior portion of the sacrum, and the narrowing of the pubic arch." Now, it is evident that deformities of this kind, which depend chiefly upon the influence of pressure exerted upon the parietes of a cavity rendered pliable by softening of the bones which form them, must be gradually produced; and that, consequently, the degree and kind of deformity must vary according to the duration of the disease, the age of the patient, and the direction in which the pressure had been most exerted, *i. e.*, whether the individual had been confined to bed, occupying chiefly a position upon one or the other side, or whether her circumstances in life had necessitated, and the gradual progress of the disease had permitted her to continue more or less, the fulfilment of ordinary employments, in standing, sitting, walking, &c. Accordingly, all writers upon pelvic distortions describe numerous grades and forms which pass in a measure, by hardly perceptible degrees of difference, into each other. Thus, Kiwisch (*loc. cit.*) describes a rachitic pelvis, which he acknowledges at first sight is not to be distinguished from a genuine osteomalacic deformity. Rokitansky (*Pathol. Anat.* vol. iii. p. 175, Sydenham Soc. ed.), speaking of the deformities induced by *mollities ossium*, says: "But these are not the invariable shapes; and the peculiar form of the pelvis" (the triangular, in which the shape of the superior aperture is like that of the heart on a card) "is not exclusively a result of *mollities ossium*, but is met with sometimes in bedridden persons who are the subjects of rickets in a high degree," and again at p. 250, in describing the deformities of the pelvis, the same distinguished author observes: "The triangular pelvis is, for the most part, a result of *mollities ossium*; but it is an error to ascribe it exclusively to that disease. *Mollities ossium* may undoubtedly be the cause of every advanced and decided degree of triangular pelvis, but minor degrees of it are sometimes due to rickets." From these quotations it is evident that these two pathologists, though among the strongest advocates for the essential difference of the two, yet virtually admit that, so far as the pelvis is concerned, the distortions induced by them are but grades of the same.

Hohl (*op. cit.*) describes and figures several rachitic pelvis, in some of which the deformity partakes of the character ascribed to the rachitic as well as that appropriated to the osteomalacic, *i. e.*, some of the bones are deformed after one fashion, and some after the other; and in two cases the appearances are not all those of the formula of the rachitic, but agree entirely with that of the genuine osteomalacic pelvis.

From these facts it would seem that, as too often happens in medical writings, a too hasty generalization has been made; the modifying influences of age, duration, progress, and stage of the disorder, habits of the patient, normal differences in the physical and chemical properties of the tissue and part implicated, &c., have not been sufficiently considered before assigning to a

particular group of symptoms or lesions a shelf apart in the nosological cabinet. And we would remark that inattention to these circumstances has not induced error with relation to the pelvic deformity alone. Rokitansky has stated (*op. cit.* p. 178) that in osteomalacia "the bone undergoes a striking change in its chemical composition, the extract produced by boiling being not only different from chondrin, but also from the animal matter of bone. Upon this last-mentioned character of mollities ossium very probably depends not only an essential difference between it and rickets, but also its malignancy; it is a very painful disease, and hitherto has never been cured." Without stopping to refute the asserted speciality of mollities ossium, as evidenced by the peculiarities mentioned in the last part of the quotation, we will simply state, on the authority of Hohl (p. 78), that if the bones of rickety children be examined at a certain period of the disease, viz., that which ushers in the reparative stage, it will be found that in these also the extract obtained by boiling is identical neither with chondrin nor with the peculiar animal matter of bone. Differences such as this should not surprise us, since we know that the chemical composition of bone varies at different ages, and even in different bones of the same skeleton. (*Vid.* paper of Dr. Rees "On the Chemical Constitution of the Bones," *Med.-Chir. Trans.* vol. xxi. pp. 406-413; *Simon's Chemistry*, vol. ii.)

Dr. Beylard next examines the pathological lesions produced in the osseous system, assigning to each of the two affections a separate chapter, and in a third the appearances found in each are compared; considerable space is likewise devoted to "fragilitas ossium." We cannot too highly commend the author for the able manner in which he has executed this part of his task. The details into which he has gone occupy more than 100 of these large and generally closely printed pages, and great industry was of course required to collect and arrange them; great candour and intelligence, too, are exhibited in their examination, and in the estimation of their absolute and relative importance. We quote the following general conclusions which he deduces from his laborious examination:—

"1. The three morbid states known as rickets, fragility, and osteomalacia, are identical in their nature, and constitute at most but three forms of one and the same nosological species; 2. If these three forms of a single disease have been separated into as many distinct affections, it has been because osteomalacia proper has been confounded with other alterations of the skeleton which are essentially different from it, and which have no other resemblance to it than the deformities which they occasion; or, because the authors of this separation have been contented with considering circumstances of merely secondary importance, such as the predominance of fragility or of softening, forgetting the improvidence of establishing distinct diseases simply upon varying degrees of a single symptom." (p. 227.)

Upon the *nature* of this interesting but obscure disease, Dr. Beylard throws no new light. Having already, in his preliminary historical sketch, alluded to several of the speculations concerning its essence, in this chapter he merely enumerates some others of the more prominent ones. In the concluding paragraph, he expresses the following cautious opinions:—

"Unfavourable hygienic conditions, an improper alimentation, are the most frequent causes of softening of the bones; its obscure and slow progress induces us to regard it as a lesion of nutrition which often induces a true cachexia. We do not deny, however, that in the course of its development an inflammatory condition may arise; the pain, the general exaltation of the animal temperature, with acceleration of the pulse, the swelling and the vascularity of the tissues, constitute a group of symptoms which it is difficult not to attribute to

inflammation. But is not this merely a secondary effect, and ought it not to be looked upon but as a means instituted by nature to repair the numerous lesions to which the whole osseous system has been subjected?"—(p. 231.)

We must certainly agree with the author as to the difficulty, at present, of unravelling this tangled skein of contradictory and conflicting statements. But we suspect that some part of the difficulty arises from the fact that several distinct diseases, apart from the scorbutic, syphilitic, and cancerous cachexias, have been represented in the motley array of bones assembled under the special banner of *osteomalacia*. We think there can be little doubt that, with reference to very many of the cases adduced, the diseased condition of the bones is to be ascribed to atrophy with fatty degeneration; such are certainly some of those specimens taken from persons of advanced age, or in whom senility had been prematurely induced, in both cases the morbid manifestations in the osseous system being accompanied with a similar degeneration of other tissues. Mr. Paget thinks that, perhaps without exception, the cases described by English writers as *mollities ossium* are examples of atrophy with fatty degeneration. ("Lectures on Nutrition," in the *Lond. Med. Gaz.* May, 1847; also, the *Pathol. Catalog. of Royal Col. of Surgeons*, vols. ii. and v.) The texture of the bones in such cases has become exceedingly porous and light, the earthy constituents are diminished, or entirely wanting, and the animal matter no longer resembles the original; "therefore, the bones are not composed of the original animal part, but a new deposit of animal substance in a new form." (John Hunter, in *Pathol. Catalogue of Royal Col. Surg.* vol. ii. p. 28.) And again, the same illustrious man, in describing a particular specimen sent to him, says: "The component parts of the bone were totally altered, the structure being very different from other bones, and wholly composed of a new substance, resembling a species of fatty tumour, giving the appearance of a spongy bone deprived of its earth, and soaked in soft fat." The medulla of the bone, too, is changed, consisting of "free oil in great quantity; crystals of margarine, free or inclosed in fat-cells; a few fat-cells full of oil, as in health; but many more empty, collapsed, and rolled up in strange and deceptive forms. The pink and crimson colours are owing to part of the oil-globules, and to the nuclei and granules in the collapsed fat-cells, being thus coloured; and there is no appearance whatever of an excess of blood in the bone, or any of its contents." (Paget; see, also, Mr. Curling's paper, *op. cit.*; Rokitansky, *op. cit.* vol. iii. p. 178; Günsburg, *Pathol. Gelehr.* vol. ii.; Gluge, *Atlas der Pathol. Anatomie*.)

Relying upon this latter statement of Mr. Paget, as to the absence of evidence of inflammation of the numerous bones which he has examined and described, we see not how to avoid admitting that many other specimens of bones of children and of adults, usually considered as indicating the existence of the specific morbid process called softening, are really indicative of the operation of an *inflammatory process*. But the microscopical and chemical examinations of the bones are hitherto so unsatisfactory upon this point as not to warrant any very positive conclusion concerning it. The majority of those who have written upon this subject agree in describing the diseased bones as being abnormally vascular, and the periosteum red, thickened, and closely adherent to the superficial laminae of bone, so much so as sometimes to drag with itself, when torn off, a layer of the expanded and spongy tissue. (Rokitansky, Beylard, &c.) But the existence of exudation or inflammatory globules in the pulpy matter found so largely in the medullary cavity and the cancellated structure of the bones, is by no means demonstrated, as a general rule, though this has been the case in some instances, and in some also pus

has been seen, as in the cases alluded to by Dr. Beylard. This opinion of the inflammatory nature of the changes receives some countenance from the pain and fever which usually accompany the disease throughout its whole progress. We should add, too, that this view is entertained by many eminent pathologists, among whom may be enumerated Henle, Mr. Solly, Schmidt, &c. The important question proposed by Dr. Beylard, and which we have already quoted, as to whether the inflammation is to be considered as the primary cause of the morbid appearances and symptoms, or only a secondary and reactionary effect, subservient to reparation, is, however, not at present susceptible of a satisfactory elucidation.

But it is not improbable that, admitting that some of the cases of rickets, or osteomalacia, are due to a fatty degeneration affecting the bones in common with other tissues, and that others again are to be ascribed to inflammation of a comparatively mild grade and of a modified character, there is yet a third group which may be regarded as the effect of some peculiarity of nutrition, different from the two which have been named, whereby a free acid is generated which liberates the phosphate of lime deposited in the bones, or prevents the proper deposition of this salt, and permits the latter to be thrown off in the urine. This acid is generally supposed to be the *Lactic*. The chief arguments in support of the supposition are: the remarkably large amount of phosphates contained in the urine of rachitic patients, leading even to the frequent formation of phosphatic calculi; the presence in quantity of acid in the alimentary canal, and the copious acid sweats. That the acid is the lactic is maintained from the fact that the disease is most prevalent in pregnant and nursing women, and in infants and young children; and that lactic acid may sometimes be detected in the interior of the diseased bones. (Schmidt, in Henle, *op. cit.* p. 371.) The earthy matter being in this way removed from the bones, and the latter being left in a diseased condition, the appearances of inflammation which they present may be the result of the reparative effort established to remedy the lesion.

The chapter on the *Diagnosis* of rickets is very well written, and presents to the reader an excellent bird's-eye view of the whole subject. Next follows a short chapter on the *Prognosis*; and the volume concludes with quite a detailed exposition of the treatment to be pursued in this most pitiable disease.

Dr. Beylard recommends most highly the cod-liver oil, in conjunction with good diet and proper hygienic means. And it would appear from his statements that the fish-oil may be with equal advantage replaced by *butter*—a pleasant exchange, truly.

In terminating our notice of Dr. Beylard's book, we must not neglect to mention that it contains a great many histories of cases of rachitic disease, which are in themselves of immense importance to the student. He has also appended a very copious bibliographical index, which contains, with few exceptions, all the desirable references to the subject of which he has been treating.

It is scarcely necessary for us to express formally our high appreciation of this work, because this has repeatedly escaped us in the course of our remarks. We now take our leave of the author, in the confident hope that the industry and intelligence which he has displayed in his Thesis will characterize and reward his future labours in the field which he has selected.

F. W. S.

ART. XXII.—*De Adipis Concoctione et Absorptione. Scripta Ed. LENTZ.*
Mitavia, 1850. 8vo. pp. 94.

De Bile Functione ope Fistula Vesicæ Felleæ indayata. Scripta REINHOLD SCHELLBACH. Mitavia, 1850. 8vo. pp. 44.
On the Digestion and Absorption of Fat. By ED. LENTZ.
The Function of the Bile investigated in a Case of Fistula of the Gall-Bladder.
By R. SCHELLBACH.

THE modification which fatty matters, taken into the stomach as food, undergo prior to their absorption by the lacteals, and the agents by which that modification is effected, are still subjects in dispute among physiologists. As fat is but slightly, if at all acted upon by the digestive fluids of the stomach, it is evident that whatever changes it undergoes to fit it for absorption must take place mainly in the intestines. Here it is supposed by some to undergo a chemical modification by its combination with the bile, with the pancreatic juice, or with certain alkaline secretions furnished by the intestinal mucous membrane; by others, it is maintained that the only change effected in it is a mechanical disintegration—an emulsion being formed by its combination with the bile, pancreatic juice, or the other intestinal fluids, either singly or collectively; while a third party consider that it is merely liquefied by the heat of the stomach and intestines, and in this state is taken up by the absorbents.

The doctrine of the chemical action of the bile upon the fatty or oily substances taken into the stomach as food—the change of these by the bile into a kind of soap—is a very old one. It is laid down by Haller, who cites as its supporters, among his predecessors and contemporaries, Steller, Floyer, Dossie, Baglivi, Vischer, Hoffman, and Homberg.

Among the modern physiologists, the same opinion is advocated by Leuret and Lassaigne, and by Valentin, although the latter does not appear to consider the saponification of fat by the bile as essential to its absorption.

The arguments adduced by Haller in proof of the action of the bile upon fatty aliment are altogether inconclusive. They are derived from the appearance of the alvine discharges in jaundice, and in obstructions of the biliary duct; from the white discharges which occur in infants, and from the fact that bile will remove the stains produced by oil. These arguments are examined in detail by Dr. Lentz, who remarks that certain modern physiologists, as for example Canstatt, in his *Manual of Clinical Medicine*, published in 1845, admit that fat is one of the excrements voided by patients labouring under jaundice and calculus of the gall-bladder. But, he adds, in all these observations, we look in vain for the chemical demonstration of the presence of fat in the excrements, for an accurate computation of the fat consumed by the patients, and for the proof of any positive connection between the presence of fat in the excrements and the disturbance of the biliary secretion. It is to be recollect that jaundice frequently occurs without any trace of fat in the discharges per anum. The fact of the occurrence of fat in the stools, Bernard adduces in evidence of a diseased condition of the pancreas.

The discharge of fat from the bowels may, however, take place independent of disease of either of the organs concerned in digestion. The digestion of fat is not effected to the same extent in every individual and at all times, even in a state of the most perfect health; consequently, when an excess of fat is taken into the stomach, the portion not absorbed by the lacteals will be found to pass off from the bowels mixed with the feces.

Dr. Lentz admits that the bile will dissolve the fatty acids which have an affinity for the alkaline portion of the bile, and hence will remove grease spots whenever the fat, by which they are produced, has become changed into an acid. This is the same explanation as that given by Bernard. "Nevertheless," remarks the latter, "it is known that bile will remove the stains produced by grease; we obtain the explanation of these facts, in appearance contradictory, when we learn that the bile dissolves the fatty acids."

"A direct experiment performed by me," says Dr. Lentz, "proves that bile will form salts with the acids of fat. By a solution of caustic potass, olive oil was converted into soap, and the alkali afterwards separated by muriatic acid; 11.5 CC. of the strained oleic acid, with the same quantity of recent ox bile, was combined by agitation, and the mixture was set aside for six days in a temperature of 35° C. After twenty-four hours, it presented three different strata, which became more and more distinctly separated. On the sixth day it was found that the inferior stratum consisted of pure bile, equal to 10 CC.; the superior stratum, consisting of oily acids, equalled 7.5 CC.; while the middle stratum was of a pale greenish colour, and equal to 5.5 CC. It was soluble in water, to which it imparted a greenish hue, and presented under the microscope a multitude of crystals. Upon the addition of a small portion of ether, it yielded a quantity of free oleic acid, and treated with muriatic acid it divided into two strata, of which the superior consisted of oleic acid, and the inferior of a solution of the muriate of soda. A similar experiment with the bile of a dog was subsequently performed with the same results.

"There can be no doubt, therefore, that the union of the bile and fatty acids forms a species of soap soluble in water; and, consequently, that, by its union with the acids of the fat in the intestinal canal, the bile may favour their absorption."

The errors and inconclusiveness of the experiments of Leuret and Lassaigne, undertaken to prove the direct agency of the bile in the digestion of the food, are very clearly pointed out by Dr. Lentz. In the experiments of Tiedemann and Gmelin, undertaken to test the power of bile to dissolve fat, a negative result invariably ensued; which is also the experience of Eberle, and more recently of Frerichs.

Dr. Lentz repeated the experiments of Leuret and Lassaigne, to show the action of bile upon crude flesh; from the result of which he infers that the loss of weight which portions of flesh occasionally undergo when subjected to the action of bile out of the body does not depend upon the solution of the fat they contain, inasmuch as a similar loss occurs when distilled water is substituted for the bile, in which latter case it is evident that no solution of fat can take place.

Some recent physiologists believe that the digestion of fatty aliment is due entirely to a chemical change produced in it by the pancreatic juice.

Bernard was the first to demonstrate that out of the animal body the pancreatic juice has power to separate the acids existing in neutral fatty matter from their bases. Frerichs refers to the experiments on this subject performed by Bernard, and to the confirmation of their accuracy by the Parisian academy, but, in adopting their conclusions, does not intimate whether these were tested by himself, upon a repetition of the experiments. Nevertheless, as Dr. Lentz remarks, these experiments have been repeatedly performed, and always with the same results; but, he adds, let us inquire whether the same result is also produced in the animal body during life by the action of the pancreatic juice upon neutral fatty substances. To this intent, three cats were fed by him upon butter and killed, the one six, the other fourteen, and the third twenty hours after feeding, and the contents of the stomach, intestines, thoracic duct, vena portae, and gall-bladder examined, but not a vestige of butyric acid could be

detected, notwithstanding all the chyliferous vessels, as well as the thoracic duct, were turgid with milky chyle.

The abdomen in two cats, that had been kept from food for thirty-six hours, was laid open by an incision along the linea alba; the duodenum, with the pylorus, were then brought out at the opening, and a ligature was applied near to the latter so as to prevent the passage of the gastric juice into the intestine. A few lines below the ligature, a small incision was made into the duodenum, through which a quantity of liquefied butter was introduced, its escape being prevented by another ligature applied above the openings of the ductus choledochus and the duct of the pancreas, so as to allow the bile and pancreatic juice to enter freely into the intestine. The duodenum being returned into the abdomen, the wound of the latter was closed by a few stitches. The one animal was killed at the termination of nine, and the other at the termination of eleven hours. In both, the contents of the intestinal canal, which had lost its normal temperature, exhaled very distinctly the odour of butyric acid, and when tested with litmus paper indicated their acid character. But in neither animal was the odour of butyric acid evident in the contents of the thoracic duct, vena portae, or gall-bladder, whether these were examined when cold or after being warmed, nor could any trace of this acid be detected when they were tested by the action of heated sulphuric acid.

In order to test whether, in the experiments just related, the butyric acid was developed solely by the spontaneous decomposition of the butter at the temperature of the animal body, in the short period indicated, or by the action of the pancreatic juice upon the butter introduced into the intestines, Dr. Lentz repeated the same experiments upon three cats, with this difference, that the upper ligature, instead of being applied near the pylorus, was passed below the orifices of the pancreatic duct and ductus choledochus, so that in these experiments not only was the gastric juice excluded from the intestines, but also the bile and pancreatic juice. Neither of the animals was permitted to partake of food for thirty-six hours previous to the operation. Through an opening made into the duodenum below the ligature above described, a quantity of butter was introduced, the escape of which was prevented by the application of a second ligature, as in the preceding experiments. One of the cats was killed seven hours subsequently, and the other two five hours later. In neither could the odour of butyric acid be detected in the contents of the intestines, thoracic duct, gall-bladder, or vena portae, nor was any trace of acidity detected by litmus paper.

Five other experiments were performed by tying the pancreatic duct in cats above its termination in the duodenum, so that all the digestive fluids, with the exception of the pancreatic juice, found their way into the intestines, and once in a cat in which the pancreatic duct and ductus choledochus were both tied above their opening into the duodenum, the bile being allowed to flow exteriorly through a fistulous opening in the gall-bladder. In four of the animals, no trace of butyric acid could be detected in the contents of the intestines, in the bile or in the blood of the vena portae; nor in the contents of the thoracic duct, in the one animal in which alone these were examined. The examination was made, in one case, six hours after feeding; in a second, fourteen hours after feeding; in a third, thirty hours after one meal, and eight hours after a second; in a fourth, forty-eight hours after one, and five after a second meal. In the fifth experiment, the animal was killed twenty-seven hours after one meal and six after a second; from the contents of the large intestines, the butter they contained was extracted by means of ether; the ether being then evaporated, and the remaining fatty matter treated by heated sulphuric acid, it gave out a very decided odour of butyric acid.

The sixth animal, in which both the pancreatic duct and ductus choledochus were tied, was killed forty-eight hours after one, and nine hours after a second meal. Neither the contents of the intestines, those of the thoracic duct, nor those of the vena porta, exhaled any odour of butyric acid.

From the foregoing fourteen experiments, Dr. Lentz draws the following conclusions:—

1st. Fat contained in the chyliferous vessels enters these, in the greatest amount, at least when nothing disturbs the normal process of absorption, in its neutral state.

2d. Fat, if it be exposed for any time to a degree of heat equal to that of the intestinal canal of the mammalia, in the absence of the pancreatic juice, may become decomposed, spontaneously, in the animal body, into its acids and their bases, as is shown by the thirteenth of the series of experiments described above.

3d. The acid of the gastric juice impedes the decomposition of neutral fat by the pancreatic juice, in the animal body.

To test the truth of these three propositions, Dr. Lentz performed a number of experiments out of the body by the admixture of pancreatic juice, gastric juice, and bile with neutral fat, as presented in the form of butter, and with starch. The results of these experiments were as follows:—

1. Although, when pancreatic juice was largely diluted by the addition of distilled water, so that the latter was as six or twelve parts to one of the former, its action upon neutral fats was not impeded.

2. Nevertheless, when a large quantity of bile was added to pancreatic juice, so that the former was as thirteen or seventeen parts to one of the latter, the action of the pancreatic juice in the decomposition of neutral fats, and in the transformation of starch into sugar, was impeded.

3. Gastric juice impeded the action of the pancreatic juice upon neutral fats, and the more so the greater the amount of acid the former contained; so that sometimes the action of the pancreatic juice was entirely suspended. This effect of the gastric juice was exhibited even when the amount of the latter was less than that of the pancreatic juice (100 parts pancreatic juice, and 74 parts of gastric juice), generally when the quantity of the latter was double that of the former, and always, when it was four times or more.

4. The admixture of gastric with the fluid of the pancreas did not impede, in the same manner, the action of the latter in the conversion of starch into sugar.

5. This effect of the gastric juice in impeding the action of the pancreatic fluid upon fatty matter was destroyed by neutralizing the acid contained in it, either by the soda of the bile, or by the addition of caustic potass.

6. The action of the pancreatic juice upon neutral fats was also impeded by the addition of other acids than that of the gastric juice, and was again restored by the neutralization of these by the soda of the bile.

Dr. Lentz remarks that, if the above deductions from the experiments performed by him are correct, it is easy to understand why, in the living body, the gastric juice, under favourable circumstances, should not impede the solution of the neutral fats by the pancreatic juice. This effect of the gastric juice being prevented, if the alkaline fluid with which it is mixed is sufficient to destroy its acidity; the pancreatic juice then being enabled to act upon the fat in the neutral contents of the intestines.

As from observation it is found that the greater portion of the fat contained in the chyle is in a neutral state, it is manifest that the power of the pancreatic juice to decompose, out of the living animal body, neutral fats into acids and

their bases, contributes nothing to explain the manner in which the lacteals are enabled to take up neutral fat from the intestinal canal.

De Lentz adds, in a note, before it can be predicted with any degree of certainty whether neutral fat will be dissolved, within the living body, by the pancreatic juice, it would be necessary for us to determine the quantity of this fluid as well as of the gastric juice, of the proper intestinal fluid, and of the bile, that is secreted during digestion, as well as the amount of acid and of alkali contained in these secretions. It is probable that the condition of the food taken into the stomach exerts, also, some influence. If it contain a considerable quantity of alkali, it may facilitate the solution of the fat by neutralizing the gastric juice; while, on the other hand, if it contain a large amount of acid, it will impede the action of the pancreatic juice in its action on the fat.

Dr. Matteucci, of Pisa, reports some experiments to prove that the fatty portion of the aliment of animals undergoes a change to adapt it for absorption by the action upon it of the alkaline fluids secreted by the intestinal mucous membrane. His first experiment was performed by adding olive oil to a weak solution of an alkali, and then exposing the mixture to a temperature of 35-40° C. The mixture assumed a milky appearance, and separated into two strata, of which the upper remained opaque and contained globules of oil; the inferior stratum was less opaque, and had in all respects the appearance of milk. With this "emulsion" the doctor filled a portion of intestine, which, after securing the ends, he immersed in a similar weak alkaline solution, and exposed the whole to a temperature of 35-40° C., when, in accordance with the laws of the diffusibility of fluids, the contents of the intestine became mixed with the alkaline solution without. A second experiment similar to this was performed with a like result. An endosmometer, formed of the urinary bladder of an ox, was then filled with a weaker alkaline solution, and immersed in the emulsion above described. The emulsion passed into the alkaline solution, and elevated the column of liquor thirty millimetres.

In a third experiment, two funnels of equal size were filled with sand; upon this, in one, water was poured, and upon the other an alkaline solution, until these fluids passed entirely through the sand. An equal quantity of oil was now slowly poured upon the sand in both funnels. The oil was quickly imbibed by the sand imbibed with the alkaline solution, but remained at the surface in the one containing the sand moistened with water.

Dr. L. frankly acknowledges that he cannot understand the idea Dr. Matteucci desires to convey by the term emulsion, used by him in the detail of his experiments. An emulsion, it is well understood, designates simply a minute mechanical distribution of oily particles throughout an aqueous fluid, by the intervention of some viscid matter which is miscible with both oil and water. Now it is evident that in the experiments of Dr. M. no such mechanical distribution of oil in an aqueous fluid could have taken place; on the contrary, a saponification of the oil was effected, as is always the case when oil is mixed with an alkaline fluid and exposed to an elevated temperature. If to this opinion it be objected that, in the first experiment, globules of oil were still present in the upper stratum of the mixture, the cause of this phenomenon can readily be explained by the fact that the alkaline solution was not sufficiently strong to convert the whole of the oil into soap; consequently, the portion that remained unchanged was found floating in globules upon the surface. *

In regard to the experiment adduced to exhibit the endosmose of fat, this exhibits nothing surprising; between the solution of soap and that of alkali there exists a chemical affinity, and consequently their commixture is

readily explained by the laws of the diffusibility of fluids. But neither of Dr. M.'s experiments contributes anything towards the explanation of the process by which fat is adapted for absorption into the chyliferous vessels, inasmuch as the greater portion by far of the fat contained in the latter has been shown to be in a neutral form.

Now, Dr. L. concludes, it must be evident that, by the experiments he has detailed, the opinions which involve a chemical change of the neutral fat taken into the stomach as food, either by the bile, the pancreatic juice, or by the proper fluid of the intestines, in order to fit it for absorption by the lacteals, are refuted. Because, 1st, such a change has been shown not to have taken place in the fat contained in the chyle, or at least in by far the greater portion of it, by the chemical and microscopic examination of the contents of the lacteals.

Because, 2d, it has been proved that no one of the secretions by which such a change could be alone effected—neither the bile, pancreatic juice, nor the fluids secreted by the mucous coat of the intestines—has the power to decompose fat so as to separate its acids from their bases. But, on the contrary, in the living animal body, provided all the functions are in a normal state, the acid of the gastric juice prevents its occurrence.

By several modern physiologists it is supposed that the only change that fat undergoes, to adapt it for absorption by the lacteal vessels, is a minute mechanical division—its minute mechanical distribution throughout one or other of the fluids of the intestines, with which fluid the fat is supposed to form an emulsion.

Bernard supposes that it is the pancreatic juice by which this mechanical distribution of fat is produced; others ascribe this effect to the joint action of the bile and pancreatic juice, and others, again, to the proper fluid of the intestines.

These views are examined in some detail by Dr. Lentz, by whom the accuracy of the arguments and experiments adduced in their support is very carefully tested. By a series of experiments performed upon living animals, he has shown that the ligation of the pancreatic duct, or of the ductus choledochus, or of both, does not prevent the absorption of fat by the chyliferous vessels, while, indirectly, he proves, also, by a long series of experiments, that the supposed minute mechanical distribution of the fat taken as aliment is not effected by any or all of the other fluids usually present in the cavity of the intestines during life.

Boussingault performed a series of carefully conducted experiments upon ducks, to determine the quantity of fat absorbed from the intestines within a given period. From these experiments it would appear that the quantity of fat absorbed in one hour was very nearly the same, however abundantly the food may have been charged with fatty matter; thus, when the animals were fed on cacao, which affords about one-half its weight of a butyrateous matter, upon lard, or upon butter mixed with rice, the quantity of fat absorbed was limited to about eight decigrammes in the hour.

Now, as in all of these experiments the quantity of water taken by the animals which were the subjects of them, and the quantity of fatty matter consumed by them varied, how can the fact of the same amount of fat being taken up by the lacteals within a given period be explained upon the supposition that it is necessary to fit it for absorption that the fat should first be formed into an emulsion with some aqueous fluid? If this latter opinion be well founded, it would be reasonable to infer that the quantity of fat absorbed from the intestines would be in direct proportion to the quantity that

is formed into an emulsion within a given time, and which of course must vary with the quantity of fluid, as well as of fat, present in the intestinal canal. It may be affirmed, however, that the amount of fat formed into an emulsion adapted for absorption depends entirely upon the quantity of the agent through the intermedium of which its mechanical diffusion in an aqueous fluid is effected, and that this agent, existing either in the bile, pancreatic juice, or in the proper fluids of the intestines, or formed by a combination of one or more of these, is, under normal circumstances, furnished always in very nearly the same amount. But, if this be true, then whatever diminishes the quantity of this agent by which the emulsion of the fat is supposed to be effected, must necessarily diminish the absorption of the latter. To test this, Dr. Lentz has performed a number of very interesting experiments.

From these, it appears that, in animals fed upon aliment containing a large amount of adipose matter, there is absorbed from the intestines, within a given period, very nearly the same amount of fat, relative to the weight of the animal, under all circumstances. That is, whether the bile and pancreatic juice are freely secreted, or one or both of these fluids be prevented from entering the intestines. Hence we cannot assume that, to adapt it for absorption, the fat taken as aliment must be first converted into an emulsion by either or both of them. The agency of the proper fluid of the intestines, to which Valentin and Frerichs ascribe the office of converting fat into an emulsion, cannot, it is evident, be tested by direct experiment; but, as the only property possessed by it that can adapt it to the emulsion of fat exists also in the bile and pancreatic juice, and in the latter to a much greater extent even than in the fluid secreted by the intestinal mucous membrane, it must be evident that the influence of the latter in causing such emulsion cannot be superior, if equal, to that of both the others; and yet, although their exclusion must necessarily cause a less amount of the fat taken into the alimentary canal to be formed into an emulsion, still, as the experiments of Dr. Lentz prove, the amount of fat absorbed suffers no diminution.

The only remaining doctrine in relation to the state in which fat is absorbed by the lactiferous vessels, is that, in its neutral state, when fluid, it is absorbed without further change; but when solid, all that is necessary to prepare it for absorption is that it be liquefied by the heat of the intestinal canal. This is the opinion of Wagner, who supposes that, during digestion, the fat in a liquid state is absorbed by certain portions of the internal surface of the intestines while the aqueous fluids are taken up at other portions. Frerichs supposes the epithelial cells at the apices of the intestinal villi to preside over the absorption of fat.

Weber believes that, besides the epithelial covering of the villi, there exists a stratum of cells of a globular form, some of which possess the power to absorb a white non-pellucid liquor, while others absorb a pellucid fluid like oil.

The opinion of Wagner, that during digestion the function of absorption is so distributed over the internal coat of the intestines that at some points always fat, and at others always aqueous fluids are taken up, affords no explanation, according to Dr. Lentz, of the manner in which fat is absorbed. It appears to him more probable that certain cells, or congregations of cells, have the sole faculty of absorbing fat, and which never receive any aqueous fluids. Such a hypothesis is perfectly consistent with the fact that a certain amount of fat, in proportion to the weight of their bodies, is absorbed from the intestines of animals in a given space of time. We are only to suppose that this fat-absorbing apparatus is limited in extent, varying only in proportion to the size of each animal, and we can readily understand that it will be able to take up and transmit only a certain quantity of fat in a given period of time.

The general conclusions legitimately deducible from the experiments of Dr. Lentz are:—

1. That the neutral fats are taken up by the lactiferous vessels, without any chemical change being previously effected in them by either the bile, pancreatic juice, or proper secretions of the intestinal tube.
2. That the minute mechanical division of fat in the tract of the intestines, or its change into an emulsion by the bile, pancreatic juice, or intestinal secretions, is in no degree necessary to fit it for absorption.

The following conclusions are, to say the least, rendered probable by the experiments performed by the author:—

1. That the change effected in neutral fats by the pancreatic juice out of the animal body—that is, their decomposition into their acids and bases—is impeded in the intestinal canal by the acid of the gastric juice.
2. That the quantity of fat absorbed from the intestines of any given animal, within a certain period, is limited; and that, provided a sufficient amount of fat is taken into the stomach, this quantity, under a normal condition of things, is at all times very nearly the same.

Still further experiments are required to determine:—

1. Whether any, and how much, fat is absorbed in the stomach itself.
2. Whether it is absolutely necessary to the absorption of fat by the chyliferous vessels that it be liquefied by the heat of the intestines.

The entire essay of Dr. Lentz is replete with interest. The several series of experiments appear to have been well devised and carefully executed, while the conclusions based upon them have all the semblance of legitimate deductions. It is true that, in regard to the conclusions drawn from those experiments in which the abdomen of the living animal was laid open and ligatures were applied to the pancreatic duct, the ductus choledochus, and to the duodenum, it may be objected that the results obtained cannot be assumed to represent positively the manner in which fat is absorbed under the ordinary circumstances of health, and while all the digestive organs remain in their normal condition; still, the experiments are so varied, in different cases, as in some measure to obviate the errors that might be expected to result from the disturbance of, or the unnatural condition of the functions produced by them.

We should have been pleased to present our readers with a detailed account of the several series of experiments instituted by the author, to enable them to judge of the correctness of the general conclusions he has drawn from each. But to have done this would have extended this notice of Dr. Lentz's work to a most unreasonable length. Every particular of an experiment must be given in order to present fairly its accuracy, and its true value in the settlement of the question to solve which it was undertaken.

The deductions of Dr. Schellbach, in regard to the function of the bile, are based mainly upon the result of observations made in the case of a dog in which an artificial fistula of the gall-bladder was formed, so that all the bile was discharged externally. These observations are limited in extent, and do not appear to sustain the conclusions drawn from them by the author, at least in reference to the main question investigated by Dr. Lentz. Dr. Schellbach believes that the bile is the essential agent in effecting the absorption of fatty matters from the intestines; but we confess that we cannot see that this position is absolutely proved by any of the facts connected with the experiments detailed by him, and upon which it is mainly based.

D. F. C.

ART. XXIII.—*The Principles and Practice of Surgery. Illustrated by three hundred and sixteen engravings on wood.* By WILLIAM PIRRIE, F. R. S. E., Regius Professor of Surgery in the Marischal College and University of Aberdeen; Surgeon to the Royal Infirmary, &c. &c. Edited, with Additions, by JOHN NEILL, M. D., Surgeon to the Pennsylvania Hospital, Demonstrator of Anatomy in the University of Pennsylvania, &c. &c. Philadelphia: Blanchard and Lea, 1852: Octavo, pp. 784, including an index.

This is a very handsome American edition of an already popular production, albeit from a new competitor in the field of British surgical instruction. The favour with which it has been greeted on both sides of the Atlantic will more than justify the author's venture and its immediate re-issue in this country, whilst it proves that he has struck upon a vein that was not by any means exhausted. Notwithstanding the Californian profusion with which works on surgery in the shape of reprints and translations, as well as native publications, have been pouring from the press, we are glad to find some prospect left for elementary text-books on this prolific subject. The practitioner may revel to his heart's content, or entire confusion as the case may be, amidst all kinds of operative surgeries, clinical surgeries, and minor surgeries; of cyclopedias, dictionaries, and most learned and voluminous "systems"—not to mention the multitude of monographs, contributions, and special courses; but in spite of all this embarrassment of riches, what is to be done or has been done for the bewildered tyro—nay, even for the older student, if not for the almost equally puzzled tutor? We fear their wants have not been hitherto provided for in the manner which the experience and sense of either party would lead him to prefer at once, were he so fortunate as to be allowed an opportunity of choosing for himself. There is hardly within the beginner's reach a volume that undertakes to tell him effectually what he ought to learn before he is prepared to grapple with discussions which he has no time at first to profit by. He cannot find one which will afford him, in a condensed but not too meagre form, a clear, methodical, and comprehensive view of the leading facts and principles of surgical pathology (*internal* as well as *external*), diagnosis, prognosis, and treatment, such as he hears, or ought to hear, inculcated in their full length and breadth at the hospital and the lecture-room.

We do not ask for actual *primers*. These, as everybody knows, are sufficiently abundant—thanks to the eagerness with which such labour-saving inventions of the enemy always will be sought for, and which the very lack above expressed of proper substitutes contributes to increase; but we would be pleased to see a greater interest shown by competent authorities in the preparation of what alone can drive the catechisms, *et il omne genus*, out of decent company. Our prayer is for an intelligible, attractive form of introductory or primary class-book, elementary in character, but imparting a thorough general groundwork, and a groundwork only, of the science and art of surgery in its foremost state of progress.

The excellent compendium of Mr. Druitt seems, so far as we can now remember, to be the work which of all others in the English language has most nearly met this want, and which has probably in consequence enjoyed the most encouraging success. The compact form and *omnium gatherum* peculiarities of text and illustration that characterize this book, together always with the really great merit of its general style and teaching, doubtless materially aided in establishing its unprecedented circulation. Still, we are inclined

to attribute some share of the result to the absence of competition from the kind of work we have been trying to describe. For the very fulness of Mr. Drift's "system," arising from the attempt to treat of everything and to illustrate everything at so small a cost of ink and paper—not to say of effort to the reader—has rendered it, after all, too much the mere *vade mecum* it was originally intended and denominated by its author—a *vade mecum*, moreover, decidedly more useful to the accomplished under-graduate and the practitioner than to the inexperienced learner.

Professor Pirrie does not pretend to address himself to any class of readers, or to place his work in competition with any others of its kind. It was prepared for the use of the members of his class, and in compliance with their wish, repeatedly expressed, to be furnished with a compendium of his lectures. In thus answering their call, it was his "endeavour to combine simplicity of arrangement, and conciseness and clearness of description, with the elucidation of sound principles and practice." A pretty full examination of his pages has led us to believe that he has succeeded admirably in his modest enterprise; at least, so far as he has gone. So admirably in many respects, and so entirely in many portions of his book, does he fulfil our hopes, that we seriously regret that his aim had not been higher in its elaboration for the press. A very little more care and time devoted to a few of the chapters, and to the discussion of a few additional topics in the same able manner that distinguishes the rest, would have freed it from inequalities and omissions which have justly been complained of, and would have rendered it one of the best text-books on its branch, and by all odds the very best *elementary* class-book of the kind we think desirable, in the English language.

The faults of Professor Pirrie have been generally admitted to be for the most part errors of omission only, and hence less detrimental to the value of his compend in the hands of students. Some of these omissions have been very well supplied, as we may show hereafter, by the American editor, who has added a considerable amount of valuable matter, compiled from the best sources in the country. The existence of others of the alleged short-comings may be a question of opinion in which the professor is entitled to the benefit of the doubt. On the whole, therefore, there is so little to object to in the doctrine and arrangement of the work, and so much to approve in it as a lucid elementary exposition of the present state of the principles and practice of surgery, and as a safe and attractive guide to the student of this branch, that we do not hesitate to recommend it warmly to our readers as already the best of its kind yet published. Our hope is that, in the new edition which must soon be called for, its author and editor may take the little trouble that would be required to place it not only in the competition which is now disclaimed, but before all competition with any publication of its class. Meanwhile, we propose to take a rapid glance at the general method and construction of our author, and at his matter here and there, taking occasion to offer by the way some extracts for the better information of our readers in relation to his general style and tone. More than this cursory view we cannot undertake, as any particular discussion of the doctrines of such a work would answer no good purpose, while a lengthened analysis of its contents would carry us beyond our present limits.

The book, then, is divided into twenty-five chapters, each of which is distinct in itself, and occupied with a particular topic or class of diseases. The headings of these chapters will serve to give an idea of the contents as a whole, and may be enumerated in so many words, and in the order of succession adopted by the author. The first ten chapters are devoted respectively

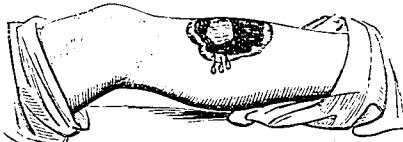
to the consideration of Inflammation and its Results (in 29 pp.); Erythema and Erysipelas (pp. 10 $\frac{1}{2}$); Wounds (pp. 31, including additions by *Am. Ed.*); Burns (pp. 21); Fractures (pp. 75); Injuries of the Head (pp. 26, including Fractures of Face by *Am. Ed.*); Dislocations (pp. 57); Affections of the Osseous System (pp. 39, including additions by *Am. Ed.*); Diseases of Joints (pp. 34, including Ankylosis and Hysterical Disease of Joints by *Am. Ed.*); and Curvatures of the Spine (pp. 23). The next ten chapters are occupied with Talipes (pp. 18); Affections of the Arteries and Veins (pp. 49); Hernia (pp. 56); Wounds of Abdomen (pp. 6, including Paracentesis Abdominis); Calculous Disorders (pp. 47, including additions by *Am. Ed.*); Affections of the Testicle (pp. 16); Affections of Genito-Urinary Organs (pp. 29); Amputations and Resections (pp. 26); Deligation of Arteries (pp. 16 $\frac{1}{2}$); and Affections of the Rectum (pp. 8 $\frac{1}{2}$). The concluding five chapters are designated as follows: Affections of the Eye and its Appendages (pp. 53 $\frac{1}{2}$, including Strabismus by the *Am. Ed.*); Affections of the Nose (pp. 10, including Rhinoplastics and Affections of the Antrum); Affections of Mouth, Throat, Windpipe (pp. 32 $\frac{1}{2}$, together with Wounds of Neck and Chest condensed by *Am. Ed.* from *South's Chelius*); Tumours (pp. 19); and Affections of the Breast (pp. 5 $\frac{1}{2}$).

We have taken the pains to ascertain the number of pages belonging to each chapter, in order to enable those, who may have the curiosity to note the difference in the amount of attention paid to the different topics, to make their own comparisons. It must be remembered, however, that the estimate can only be approximate on account of the different numbers and sizes of the marginal wood-cuts. Still, it must be confessed that the disproportion is obvious in more than one instance upon the slightest examination. Certain topics are dispatched in a hurried and very unsatisfactory manner, while others are dwelt upon to an extent that would not be expected in such a volume. Inflammation and its Results are crowded into twenty-nine pages, Erythema and Erysipelas into ten and a half, Wounds into thirty-one, and Fractures into thirty-one, while the article on Burns is allowed to take up twenty-one pages, that on Curvatures of the Spine twenty-three, that on Calculous Disorders forty-seven, and that on Talipes eighteen. We do not wish to say that there is one word too much in a single chapter. Some of them are admirable specimens of condensation, and contain extremely useful summaries. But we cannot help thinking that the balance might be better struck in many places, and that an importance has been given to several sections, which more properly belonged to others.

Chapter 1st, on Inflammation and its Results, although enriched with the latest pathological views, and remarkably full, considering its very narrow limits, is disproportionately brief, especially in its history of the Results. In regard to the treatment, it is altogether too deficient. Under the head of Ulceration may be observed six different wood engravings of the leg, intended merely to exhibit the different varieties of ulcer. They are certainly very becoming specimens of zylographic art, but afford too vague an idea of the morbid appearances they are intended to depict to warrant the amount of room thus withdrawn from other objects in their immediate vicinity. We would rather have the roughest kind of a diagram, if it would only convey a definite idea, than the most superior drawing; and under this impression it seems to us that a magnified or exaggerated delineation of the ulcerated surface by itself would answer a better purpose than a larger picture, although this latter might represent a handsome foot and leg. While upon this subject, we are tempted to make our first quotation from page 52, on *granulation*:-

"Granulation."—The process by which the cavity is filled up, and continuity of tissue restored, is called *granulation*; it consists in exudation from the surface of the cavity of blood-plasma, constituting the fluid *cytoblastema*. Part of this blood-plasma degenerates into pus-corpuses, but part becomes transformed into nucleated cells. Minute granules, forming the nucleoli (as they are called), are developed, and to the assemblage of these the term *nucleus* is given. On the nucleus a cell-wall becomes developed, which, at first closely embracing the nucleus, is afterwards raised up from it, and the nucleus thus separated from the cell-wall occupies an eccentric position within it. In these organized products, vessels are formed, and the whole, when thus developed, constitutes a layer of granulations, spreading over the surface of the cavity, and giving it the appearance of being covered with innumerable small bodies of a conical form, and of a florid red colour. From the granulations blood-plasma is exuded, part of which degenerates into purulent matter for the defence of the granulations, and part is transformed into nucleated cells, by which a new layer of granulations is formed. The cells of the first-formed layer undergo further changes, and are ultimately developed into the texture of the part, from the vessels of which the exudation of blood-plasma took place, and each subsequent exudation furnishes a *cytoblastema* for the formation of purulent matter and nucleated cells. By the successive formation of these cells, by their becoming ultimately developed into permanent tissue, and by the centripetal contraction of the original textures, the cavity is filled up, and the next part of the process is *cicatrization*, or the formation of cicatrix. This usually begins when the granulations arrive on a level with the surrounding skin, when the blood-plasma, hitherto converted partly into pus-corpuses, and partly into nucleated cells, passes into cells which, by the process of development, are converted into fibres, and constitute the cicatrix. The new skin usually takes its rise from the margins of the old skin; but in some few instances, portions of new skin are seen forming on the surface of the granulations, like little islands, quite remote from the margins. Some have endeavoured to account for this fact by the supposition that the old skin has not been completely destroyed, as we not unfrequently find in burns, and that the isolated portions of new skin spring from the parts not entirely destroyed by the burn or ulceration; but I am convinced by various cases which have come under my own

Fig. 8.



observation, that this explanation is not satisfactory. I shall only refer to one case, that of a young lady, whom I had the opportunity of seeing, together with one of my colleagues in the University. The lady was the subject of phagedenic ulcer, of considerable size and of great depth, in the leg; and as other means had had no effect in arresting the destructive action, the whole surface was destroyed to a considerable depth by pure nitric acid. After the removal of the slough, healthy action took place, and a large isolated portion of the skin formed in the middle, and gradually increased until it joined that formed from the circumference of the ulcer. I have for several years been in the habit of showing to my class in the University, a drawing of this case, as it is an incontrovertible instance of an exception to the ordinary rule of the formation of skin from the circumference only, and an evidence that the explanation mentioned above is not satisfactory."

The same deficiency that was remarked above in regard to Chapter 1st, though not quite so decided, appears in the section on *Erysipelas* in Chapter 2d. Chapter 3d, on Wounds, although brief, in some respects is capital. We have room only for one extract that may appropriately follow the one already given:—

Treatment for Granulation.—Wounds may require to be treated for granulation, either when inflammation has proceeded too far in a case which it was first attempted to heal by adhesion, or when, from the beginning, it was evident that granulation was the most suitable mode of healing, whether owing to loss of substance preventing coaptation, or to extensive contusion, or to the presence of foreign matter which could not be removed; all of which conditions are incompatible with the attainment of adhesion, on account of the active inflammation to which they give rise.

In the former case, that of a wound treated hitherto for adhesion, its edges become swollen, red, and painful. Swelling more deeply seated causes the margins to separate, and purulent matter is soon poured forth. The indication here is to repress inflammation; in fulfilment of which, all sources of local irritation and general stimulation must be withdrawn. Sutures, if present, should be removed, and only a few strips of plaster left, to prevent any unnecessary gaping of the wound, and in many cases they also must be dispensed with. To the parts thus relieved from every kind of local irritation, warm water-dressings are applied, or a light, soft, moist, and warm poultice, if that application be still employed.

In the second case where granulation is from the first considered to be the most available mode of cure, the treatment is essentially the same as that mentioned above. The part is elevated, and kept at perfect rest; no sutures are employed, and only a few strips of plaster are used to connect the more loose portions of the wound. Cold water-dressing is applied till oozing of blood ceases; it is then gradually changed to the tepid, and next to the warm dressing, as the vascular action rises, so as to soothe and relax the tumefying wound. When the inflammation proves so active as to threaten gangrene, it must be repressed by local bleeding, and if absolutely necessary, by general depletion, in addition to the antiphlogistic regimen, which, during this stage of acute inflammation, is to be adopted.

In both cases, the same point is now reached. Warm dressings are continued so long as inflammation remains active; but as it subsides, the heat of the dressing is gradually lowered until it be again merely tepid, or even cool.

Under this treatment, the surfaces, if matters go on favourably, become clean in a few days; granulations spring up, and healing advances. The discharge which, during the height of the inflammation, had been very profuse, and far from laudable, now diminishes in quantity and improves in quality. In these circumstances, the wound, when superficial and broad rather than narrow and deep, requires merely the treatment proper for an ordinary ulcer; comprising the water-dressing, medicated, when necessary, with metallic salts, to stimulate indolent granulations, and the employment of carefully-adapted pressure by a bandage, when necessary to repress oedematous swelling.

But if the wound be deep, without much loss of substance—in fact, such a case as would have healed by adhesion had not inflammation prevented—then, at this stage, when the surfaces are granulating well, and secreting little pus, they will, if placed in mutual contact, speedily cohere, affording a most satisfactory and rapid cure by secondary adhesion. Plasters are employed to retain the parts in apposition, and a bandage, lightly and uniformly applied, is in general necessary to give support. As absolute local rest is necessary for healing, any neighbouring joint, which interferes with this essential condition in the wound, must be prevented from exercising its natural functions, by a splint fastened with a few turns of a roller, or with a buckle bandage applied at two or more points, lightly, so that no oedema may ensue on the distal aspect, and arranged so that neither splint nor bandage shall compress the injured parts, or come in the way of the requisite dressings. Cleanliness is throughout attended to; the actual edges of the wound are not touched, but

from all around them the discharge is frequently wiped away with a small dose of clean lint, tow, or rag, or indeed of anything clean, soft, and absorbent. A good sponge answers well when there is only one wound to dress, because it can then be frequently washed; but, in hospital practice, it would come into contact with all kinds of sores, and would never be sufficiently well or often cleansed after each time of its employment. On the other hand, the morsel of tow, being of small value, may be destroyed, and a fresh piece employed on each occasion."

Chapter 4th, on Burns, as already intimated, is admirable. It is evidently written *con amore*, and with no ordinary hand. There is no part of it that would not create a favourable impression. We must leave it with one quotation:—

"The *Constitutional Treatment of Burns* comprises *five* indications: namely, to promote reaction; to control and regulate its intensity; to watch for and treat inflammatory affections of the internal organs; to support the system under hectic and its complications; and lastly, to remove mechanically, under certain circumstances, the cause of the hectic.

"The first indication, then, of general treatment is to promote reaction. It is said that reaction is mainly brought about by the severity of the pain; but this must only be when the pain is under a certain degree of intensity; for it is well known that this symptom, when excessively severe, itself exercises a powerfully depressing influence upon the heart's action. In many cases, nature is able herself to rouse the system; but if not, reaction is to be promoted, when the patient can swallow, by the administration of some of the diffusible or more permanent stimuli, such as ammonia and brandy, in small and frequently-repeated doses. When the general surface and the extremities are cold, warmth should be applied, and, if practicable, sinapisms to the feet and pit of the stomach; but the effect of these must be closely watched, lest they induce sloughing. Opium has been recommended in large doses, to diminish the pain, but this will tend to increase still further the cerebral congestion, which dissection has proved to be so common at this stage; while, on the other hand, small doses will have no effect. The pain should in these cases be relieved, as far as possible, chiefly by topical remedies. When the collapse remains long, a warm and slightly stimulant enema may be administered, and repeated if necessary.

"This stimulant treatment, during collapse, must not, however, be carried too far; and it is to be suspended so soon as signs of approaching reaction appear; for otherwise, when that stage is fairly established, the persistent effects of a superabundant stimulation, or, in other words, its surplus would exaggerate the reaction, which would now require as active treatment in the opposite direction, and when subdued, if indeed not fatal, the system would be much more enfeebled than it would necessarily have been, had nature been left a little more to her own resources during her efforts to institute reaction.

"When, as stated before, premature and asthenic reaction comes on, marked by great irritability and nervous excitement, with a rapid, throbbing, but feeble pulse, the exhibition of opium, in large doses, is attended with the most beneficial results.

"The second indication is to regulate the intensity of the reaction. When it proves excessive, without apparently any internal organ in particular being attacked, the antiphlogistic regimen will in general be sufficient. In very few burns is bloodletting in any form required, or, indeed, in hospital practice, admissible; but it may possibly be demanded, in a few cases, to relieve the general inflammatory state of the system; and, at the same time, to moderate the local action, and thus limit the amount of secondary sloughing. In having recourse to these depletive measures, however, it must never be forgotten that, in all burns, except those of the first and second degrees, the powers of the system may, at no distant day, be taxed to their utmost to support suppuration, perhaps large in quantity, prolonged in duration, and secreted by an extensive surface. Accordingly, the slightest unnecessary lowering of the system will entail a still more profuse and protracted suppuration—a still more tedious and possibly imperfect cicatrization.

"The *third* indication relates to the inflammatory affections of various organs which may occur. Their onset is often very insidious, and so must be watched for. They are to be treated in accordance with the ordinary medical principles applicable to each particular complication. Bloodletting, when necessary, should be as moderate as possible, for the reason already specified; and also on account of the well-established principle, that depletion cannot be borne to the same extent in secondary inflammations, after a shock or injury, as in a primary or idiopathic affection of the same nature.

"The *fourth* indication is to support the system against hectic, and its complications. The appropriate treatment has been described when speaking of Hectic Fever. Over-stimulation must be guarded against, as this, equally with debility, impedes cicatrization by inducing flabby and exuberant granulation. The bed-sores, resulting from long-continued pressure on the salient points of the back, sacrum, nates, and heels, exercise a very depressing effect. When threatened, a strong spirit-lotion is an excellent application; or, if already formed, the part may be pencils with nitrate of silver. Under the crust thus produced, the part, if relieved from further pressure, will readily heal. Arnott's water-bed, or a Mackintosh air-cushion, affords great relief by equalizing the pressure over the whole decumbent surface.

"The *fifth* indication is to remove mechanically, when necessary, the cause of the hectic. Amputation, though frequently performed to fulfil a merely local indication, namely, the removal of a useless limb, is sometimes necessary for the sake of the constitution. Thus, if hectic be extremely urgent, the suppuration continuing very profuse, with a large surface still remaining for cicatrization, while the amount of the former is not satisfactorily diminished, nor the rapidity of the latter materially increased by remedies, then amputation must be performed in order to save life.

"Suppuration may be prolonged, and cicatrization delayed—from debility of system—from an extensive surface being involved—from bone having become necrosed—and from the opening of a large articulation, either by the sloughing of the tissues, or subsequent to inflammation and abscess in its interior."

In relation to Chapter 5th, on Fractures, we may say that the introductory section is entirely too brief, and the whole chapter, with the exception of the section on fracture of the femur, is unsatisfactory and defective. Several important fractures receive no notice whatever, whilst others are passed over with very little mention. The history and treatment of the fractures which he does take the time to discuss, however, are, like other matters in the book, almost invariably well done. His mode of tabulating the varieties of fracture, and the different indications for treatment, strikes us as especially convenient. We are glad to see that Dr. Neill has introduced a note and drawing of Dr. Bond's recently introduced splint for Barton's fracture, and one of Fox's apparatus for fractured clavicle. Of the remaining chapters the best are—the 6th, on Injuries of the Head; the 8th, on Affections of the Osseous System; 9th, on Diseases of the Joints; 10th, on Curvatures of the Spine; 12th, on Affections of the Arteries and Veins; 13th, on Hernia; and perhaps the 15th, on Calculous Disorders.

Chapter 6th, on Injuries of the Head, is judiciously and ably written, and bears internal evidence not only of decided erudition, but ample practical experience.

Chapter 8th, on Affections of the Osseous System, is an instructive and extremely interesting chapter, in which the author again appears on evidently favourite ground. Next, we have an excellent chapter on Diseases of the Joints, and following this another, on Curvatures of the Spine, that would do honour to any of the systematic works. That on Diseases of the Joints is a remarkable history of the present state of knowledge of those intricate affections. Its illustrations are among the most curious and valuable in the whole book. The chapter on Talgipes is clear and well digested, and full

enough to bear its due proportion to the other parts. Chapters 12th and 13th, succeeding this, on Diseases of the Arteries and Veins and on Hernia, respectively, are two of the most valuable and instructive chapters in the book. The extracts here appended from the former will interest the reader, and may serve as fair examples of the manner in which its subject has been dealt with. First, let us read his sketch of

"STEATOMATOUS DEGENERATION."—This disease, named less properly by some atheromatous degeneration, was long overlooked, and seems to have been first noticed by Monro and Haller; but since they drew attention to it, it has been investigated with great success by many able pathologists, and its anatomical characters and progress distinctly pointed out. Steatomatous degeneration commences by minute granules, of a pale yellowish colour, situated between the internal and middle coats. While the disease is in this rudimentary state, the lining membrane is scarcely elevated at all; it is transparent, so that the spots are seen through it; it is unchanged in consistence; and if it be peeled off, the granules being adherent to its surface, come along with it. There is no accompanying redness, or any mark of inflammatory action in the surrounding textures. These granules coalesce into groups or masses, in the next grade of the degeneration, and the inner membrane, though unchanged as to transparency or texture, is scarcely so flat as during the early stage; and if it be peeled off, part of the deposit comes away with it, and part clings to the middle coat, from which it appears obvious that the unnatural deposit is developed between them. The middle coat, at the seat of the deposit, is of a light yellow colour, and of a more friable texture than natural, but neither in it nor in the surrounding textures can traces of inflammatory action be at this stage discovered. The deposit has the consistence of suet—feels greasy to the touch—is of a cheese-like opaque appearance, and when broken down by the finger, gives the sensation of minute granules scattered through a fatty substance. Not only has it a fatty appearance, but the researches of Gulliver show that its chemical composition also differs but little from that of ordinary fat; and this being the case, of the two appellations, steatomatous and atheromatous degeneration, the former is the more correct. Bizot detected shining particles in this deposit; Craveilhier, small masses resembling the cholesterol scales of small gall-stones. Gluge, on examining these masses with the microscope, found

them to consist of fat-globules; and Sivaine, who also repeatedly examined them with the microscope, states that he found them, on several occasions, to consist of fat-globules merely; generally, however, they were made up of an amorphous granular mass, mingled partly with fat-drops, partly with numerous cholesterol rhomboids. The shining particles are often very numerous, some having a golden, and some a silvery hue.

"The steatomatous degeneration, after attaining the grade last described, may undergo one or other of various transformations; the two principal changes, however, are ulcerous softening and osseous transformation. For the sake of a clearer description, the various changes in ulcerous softening have been arranged into three stages.

"The first is characterized by the absence of all marks of inflammation in the surrounding textures, or of any change in colour, transparency, or consistency of the internal membrane. This membrane, however, is slightly elevated, there being more of the deposit than in the former grades of the disease; and the middle coat is still more altered and softened, and of a still brighter yellow colour at the affected spot. In the second stage, the patches are distinctly elevated, like pustules, and when pressed, communicate to the finger of the examiner the

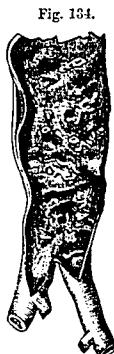


Fig. 134. From a preparation in my museum.

impression of their containing a semi-fluid substance. After the membrane has been opened and the matter discharged, it is found, on examining the place in which the matter was contained, that the middle coat presents an ulcerated appearance; but some part of it still remains between the place in which the matter was situated, and the outer coat. In the third stage, the internal membrane having cracked allows an escape of the deposit, and falls down towards the middle coat, producing a depression; or the internal membrane having (as happens in many instances) fallen off, or having disappeared, an ulcer is formed, with the walls of which the sanguineous current is in contact, the edges being irregular, and the walls formed of the outer coat, or of that and the yellowish detritus of some very small portion of the middle coat. The outer coat becomes thickened, and the seat of many injected vessels, but no traces of inflammation are to be seen in the inner and middle coats at the parts surrounding the seat of the degeneration. Such are the characters of the steatomatus degeneration, when it undergoes the ulcerous transformation; but, as has been stated above, it may undergo an opposite change, termed the osseous transformation. The term transformation has been objected to, when applied to this last-mentioned change, as the calcareous matter is merely deposited in the yellow steatomatus substance, which is regarded by many pathologists as forming its nidus. The calcareous matter assumes the form of thin, brittle scales or plates, of a yellowish-white colour, surrounded at first by steatomatus deposit, but not having any fibres or organized structures between them. They are believed to increase very slowly, the surface directed to the lining membrane more slowly than that directed outwards; and they evidently enlarge more in extent than in thickness. As the calcareous deposit increases, it comes into contact with the lining membrane, from which it is in some instances separated up to this period by a layer of albuminous matter spread over it. The inner membrane in many cases at length gives way, so that the blood is in immediate contact with the calcareous deposit, and the middle coat, after having been attenuated and changed as already described, ultimately disappears, and its place may be said to be occupied by the concretion; hence has arisen the erroneous opinion that this coat itself is transformed into bone. The calcareous concretions are found in various forms; they usually consist of plates or scales, varying considerably in extent, and, in some rare instances, occupying the whole circumference of the vessel at the affected part, so as to convert it into an inflexible tube. Sometimes they consist of minute grains; and more rarely, they give, on examination with the finger, the sensation of a number of minute bodies, movable on each

Fig. 135.

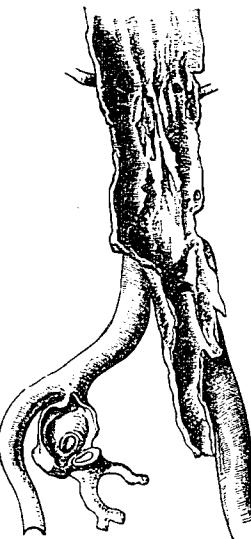


Fig. 135. From a preparation in my museum.

other, as if jointed together. As cartilaginous degeneration of the inner coat is not very unfrequently found coexisting with the form of calcareous transformation just described, the error of ascribing the calcareous deposit to ossification of the previously-existing cartilage is easily accounted for. These subjects of anatomico-pathological investigation have occupied the attention of many pathologists, and in the present state of our knowledge of them, they are considered, not as different stages of the same disease, but as entirely different diseases—the result of different morbid actions; the one commencing on the free surface of the internal coat, the other in the cellular tissue between the internal and middle membranes. Such are the anatomical characters of the ulcerous and calcareous transformations; the latter, however, may give rise to ulceration, and it may therefore be stated, that, in the progress of steatomatous degeneration, ulcerous transformation may take place, with or without osseous transformation. Sometimes when the steatomatous deposit exists in great quantity, it diminishes the channel of the artery, but it is much more frequently productive of dilatation with or without ulceration, or of rupture, or of circumscribed or diffuse false aneurism—dilatation being usually the result of the steatomatous deposit, and rupture with its consequences, of that condition when accompanied by calcareous concretion. A variety of the steatomatous deposit has been described by Scarpa, Stentzel, Craigie, and others, in which the secretion is of a yellowish colour, and of a cheesy or wax-like consistency. It commences most frequently in the bifurcations of arteries, and originates between the middle and inner coats; but it differs from the deposit already described, in being of firmer consistence, and in rarely containing gritty calcareous deposition. To this variety some restrict the term steatomatous degeneration, and give to that already described the name of atheromatous deposit."

Next we may turn to the history of aneurisms, and particularly to their

"*Spontaneous Cure.*"—Nature sometimes, though very rarely, effects a cure; and an aneurism, therefore, which is not accessible to surgical treatment, does not invariably terminate fatally. The processes by which a spontaneous cure may be effected are the following:—

"*First.*—The most frequent manner of a spontaneous cure is, by the sac becoming filled with lamellated coagulum. The various stages of this mode of favourable termination, first minutely described by Hodgson, and afterwards minutely investigated by many other competent observers, are the following: the sac becomes completely filled so as to preclude all further entrance of blood. The artery, by deposition of coagulum, becomes impervious as far as its nearest considerable branches, and is ultimately converted into a small impervious cord—the circulation in such cases being maintained by the blood, which is received into the branches given off from the arterial trunk above the aneurism, being discharged into branches given off from the trunks below, and conveyed through the last-mentioned branches by inverted circulation into the trunks from which they originate—both sets of branches becoming much enlarged. Or the artery may remain pervious, the blood passing over the closed-up sac at the part where its mouth communicated with the vessel. The tumour becomes smaller and harder in consequence of absorption. Petit records a case of spontaneous cure in which the aneurism, at one time as large as an apple, became as small as an olive. Examples of this mode of spontaneous cure are to be found in the writings of most surgical authorities on this subject. The accompanying drawing is taken from a very good example in my own collection of preparations.

"*Second.*—In some examples where the whole circumference of a vessel has become aneurismal, a spontaneous cure has been effected by a canal being left through the centre of the lamellated coagulum, through which the blood continued to circulate. There is reason to believe this to be an extremely rare mode of spontaneous cure.

"*Third.*—It occasionally happens that from over-distension, or some other circumstance, inflammation of the sac and surrounding parts supervenes, and goes on to gangrene, the whole of the aneurismal tumour sloughing away, and by that means a spontaneous cure is effected; hemorrhage from the vessels

leading to the part being prevented by the same process as when gangrene takes place in other circumstances.

"Fourth."—Another mode is by the aneurism pressing on the trunk leading to or from the aneurism, so as to obstruct the circulation. If the size and position of the tumour be such as to cause an approximation of the opposite sides of the artery either on the cardiac or capillary side, there can be no doubt that a cure will be the result. When the pressure is on the cardiac side, the cure is effected on the same principle as in one of the modes of surgical treatment described in the next section.

"Fifth."—Pressure on the trunk leading to the aneurism may be produced by other causes than the aneurism itself, as by a tumour not aneurismal, or by another aneurism on a neighbouring artery; and thus a spontaneous cure may result. Mr. Liston records an example of sub-clavian aneurism, which on dissection was found to have been cured by an aneurism of the arteria innominata.

"Sixth."—The same favourable result will follow, when inflammation takes place in the artery, and fills its calibre with coagulum.

"Seventh."—Sometimes a portion of lamellated fibrinous coagulum, becoming detached, falls into the sac, and thus causes diminution, or complete occlusion of the mouth. In the latter case, congegulation of the blood in the sac must take place, and in the former the consequent diminution of the circulation through the sac is much calculated to promote deposition of fibrin, and to accomplish a spontaneous cure. In this mode the artery may or may not become impervious.

"Eighth."—A portion of the coagulum may fall into the artery and obstruct it; thus effecting a cure. Or,

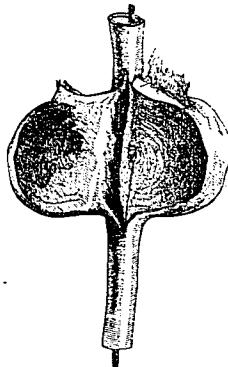
"Ninth."—The aneurism may burst and become diffuse. If the presence of the diffusely infiltrated blood do not give rise to the untoward consequences formerly described, it may, by its pressure on the cardiac side of the tumour, so weaken the force of the circulation through the aneurism, as to promote the deposition of lamellated coagulum, or to arrest the circulation of the fluid parts of the contents of the sac, and thus promote their coagulation. Such are the methods by which nature sometimes, though rarely, effects a spontaneous cure; and it may be a consolation to patients who are subjects of aneurisms in accessible situations to know that their case is not hopeless, and that a spontaneous cure is not impossible."

We begin the examination of the article on *Hernia*, with a passage descriptive of the use and *modus operandi* of the truss, taken from the paragraphs relating to the treatment.

"I. THE DIFFERENT WAYS IN WHICH THE USE OF A TRUSS PRODUCES A COMPLETE OR RADICAL CURE OF HERNIA."—*First.* If a hernia has been very suddenly produced, if it be very small, and if it be very quickly returned, the hernial sac may either return with the hernia, or be gradually drawn back into the cavity of the abdomen. The sac being empty, and no force pressing it downwards, its ascent will be promoted by the elasticity of the peritoneum lining the walls

Fig. 142. Spontaneous cure of aneurism of the femoral artery by the sac being filled with coagulum; the vessel remaining pervious. From a preparation in my museum.

Fig. 142.



of the abdomen in the neighbourhood of the protrusion, and by the stretching of the peritoneum in various movements of the body, as well as by the elasticity of the hernial sac itself. After the return of the sac, the pressure of a truss sometimes produces sufficient diminution of the opening by contraction, effusion of lymph, and consequent joining of the surfaces, to prevent any future protrusion of sac or hernia. This kind of cure, however, in which the sac returns, and its future protrusion is prevented by the diminution of the opening, is only to be looked for in small hernias of short standing, and very suddenly produced. If a hernia be of considerable size, and more especially if it be also of long standing, the distension of the hernial sac, and the pressure of the surrounding parts, excite a degree of inflammation by which those parts and the sac become adherent to each other, so that the sac cannot be returned into the abdomen, and the kind of complete cure already described cannot take place.

"Second.—After the return of a hernia, the sac, being empty, contracts by its own elasticity, in accordance with the general law that membranous parts accommodate themselves to the state of their contents. This kind of closure of the hernial sac is analogous to the contraction of the tubular portion of peritoneum, which exists within the inguinal canal for some time after the descent of the testicle. A truss, by approximating to each other the sides of the hernial sac, may assist the natural elasticity in closing up its neck, and in bringing about a radical cure.

"Third.—Occasionally, the wearing of a truss for a long time produces thickening of the neck of the sac, or of the cellular tissue surrounding it, or of both, and thus interrupts the communication between the cavities and the abdomen, and the hernial sac.

"Fourth.—The pressure of a truss often excites adhesive inflammation in the sac, by which its opposite sides become joined together by coagulable lymph, and a recurrence of hernia is prevented. This condition of parts is very frequently found in the bodies of persons who have been subjects of hernia.

"Fifth.—According to J. Cloquet, the opposite sides of the sac sometimes become adherent without the intervention of lymph, and without the very slightest traces of any inflammation. He supposes that the membrane ceases to secrete the fluid by which it is naturally bedewed; that it becomes dry, and that the sides become adherent without the intervention of any substance. In the ordinary form of adhesion of the opposite sides of a hernial sac, effusion of lymph, thickening of the sac, and traces of adhesive inflammation are perceptible; whereas in this method of complete cure there is immediate union, with thinning of the peritoneum, and the entire absence of all traces of inflammatory adhesion.

"Sixth.—Paré, Arnould, and others, record cases in which complete cures were effected by the firm adhesion of the formerly protruded parts to the peritoneum lining the abdomen around the mouth of the hernial sac; and as in these cases trusses had been worn, it was believed that the pressure gave rise to inflammation in the neck of the sac, and that this inflammation, having extended to the membrane lining the cavity of the abdomen, produced the adhesions.

"Seventh.—Absorption of the neck and part of the body of the sac sometimes produces radical cure. Surgical observers have described this condition of parts, and I lately had an opportunity of demonstrating it to the students at the School of Medicine in Marischal College, in the body of a person who had worn a truss for many years for the cure of a reducible hernia. Almost the whole of the neck and the upper part of the body of the sac were absorbed, but the remaining portion of its body and fundus were entire, and formed a bag in the scrotum in front of the tunica vaginalis.

"II. THE PRECISE SITUATION TO WHICH THE TRUSS SHOULD BE APPLIED.—Since the immediate object which the surgeon desires to accomplish by the pressure of the truss is, to prevent a return of the hernia, and the ultimate object, to induce some of the various changes already described, by which the tendency to its recurrence may be removed, it must be evident that the precise part to which the pressure should be applied is, that where the hernia first quits the abdomen. This point will vary in the different forms of hernia, and

will afterwards be explained; but meanwhile it may be stated that before the various changes in the sac, and in the opening by which it quits the abdomen, were clearly understood, by which changes a complete cure of hernia is effected, a very common error which prevailed was, to apply the truss too low instead of exactly over the opening; and in consequence, the advantages of the truss were often not obtained, and, moreover, various inconveniences, which will hereafter be described, were frequently produced.

“III. THE LENGTH OF TIME A TRUSS SHOULD BE WORN.—As the prospect of a complete cure is very different at the different periods of life, it being almost a matter of certainty in young persons, occasionally met with in adults, and not to be expected in elderly persons, there will be a corresponding difference in the length of time that the truss must be worn, as well as in the object of wearing it; the object being at one period merely preventive or palliative treatment, at others palliative treatment and radical cure. In young persons, a complete cure is often effected in less than twelve months, in adults seldom under two years at least, and in old persons it is not to be expected. In regard to the time a truss should be worn, Sir Astley Cooper remarks: ‘You will be asked by the patient when you have applied the truss, how long he is to wear it; tell him to wear it at least two years. He will then ask you whether he is likely to be cured at the end of that time; your answer must be that this must depend upon his age. A young person is generally cured at the end of two years, but it will be advisable for him to continue to use the truss for three years. If the person be not young, there is not much hope of effecting the cure of hernia by wearing a truss.’

“The truss should be constantly worn, not only during the day, but also during the night, because, although the probability of a recurrence of the hernia is by no means great in the recumbent posture, yet it might be induced by a cough, or any sudden change in the posture of the body in bed, and then the cure would require to be commenced anew from that period. It ought also to be kept in mind that the recurrence of the hernia, after the use of the truss has been commenced, is attended with more risk than before, because if thickening about the neck of the sac or around it has commenced, the hernia is more likely to be irreducible from being surrounded by firmer textures. According to some of the best surgical authorities upon this subject, the only exceptions that should be made to the constant use of the truss are, when it is first applied, and before it is to be laid aside, when it is believed that a complete cure has been effected. When first applied, the truss frequently gives rise to irritation, and heat of the skin, with inconvenience and discomfort from pressure and restraint; and until these unpleasant sensations wear off, which will usually be in a week or two, it may be discontinued during the night; but the patient should even then be careful not to remove it until he is in the horizontal posture, and he ought again to apply it before raising himself from that position. And when it is believed that the cure is complete, the truss may be discontinued at night, before it is entirely laid aside. For some time after the daily use of the truss is discontinued, it is a judicious precaution to wear it whenever the body is more than usually relaxed. When the danger of a recurrence of the hernia is greater, or during any unusual exertion, and during the whole period of wearing the truss, it is proper, on making any violent movement or effort, to afford a degree of support by the hand over the pad of the truss. When it is believed that the cure is complete, the surgeon should make a very careful examination of the part where the hernia came out from the abdomen; and before he sanctions the discontinuance of the truss, he should endeavour to ascertain that no swelling can be felt, and that during coughing, or any exertion of the muscles of the abdomen, there is no sensation of a hernia striking against the finger when applied to the opening.”

We quote these passages because of their decided testimony in behalf of the possibility of radical cure by means of trusses—a possibility sustained by every surgeon entitled to authority, and of which our own experience has long since convinced us beyond the shadow of a doubt.

It would give us pleasure to present his discussion of the question of open-

ing the sac, which he seems to have studied with unusual care, and has managed to exhibit in a clear and instructive light. Our narrowing space, however, restricts us to a quotation—this time in relation to the abuse of *taxis*. The important lesson here conveyed cannot be too early or too forcibly impressed upon the learner's memory. And, accordingly, Prof. Pirrie's earnest warnings will be found to bear the stamp of his practised judgment and eminently cautious mind, whilst they are supported by the authority of the ablest surgeons of the day.

"There can be no doubt that intestinal inflammation is the most frequent cause of death after the operation for strangulated hernia. Some of the advocates of Petit's method have assigned as the causes of that inflammation, when the ordinary proceeding is adopted, the exposure of the intestine to light and air, change of temperature, and handling. I agree with Mr. Lawrence in ascribing it not to these agents, but chiefly to the long-continued pressure of the stricture, owing to the operation being *too long delayed*, and to an injudicious and *too frequent use of the *taxis** previous to the operation. I remember being very much struck with an observation of Desault's; I have not his works beside me at present, but it is to this effect: 'Think well of that hernia which has been little handled and soon operated on.' The operation is justifiable and necessary, when the patient has been brought fully under the influence of chloroform, and the *taxis* has been fairly, fully, and skilfully tried, without producing the desired effect. The conviction being thus produced, that by no other means than an operation is there hope of saving the life of the patient, it ought to be resorted to as quickly as possible. Much handling must not only give unnecessary pain, but also increase the risk of hurrying on the inflammation to results which, even though the operation should be performed, would render it unsafe to return the hernia. When, therefore, the *taxis* has been fairly and skilfully tried on a patient fully under the influence of chloroform, no advantage can, but considerable injury may, result from the repetition of treatment already found to be unavailing. Many considerations show that the operation should be performed as soon as possible after its inevitable necessity has been found to exist. Delay, like undue handling, increases the risk of inducing such a state of the hernia, in consequence of inflammation, as would render its return unsafe. From the circumstance that a hernia may speedily prove fatal, and from the depressed state which comes on in consequence of delay, rendering the patient less able to stand the shock of an operation, will be seen the importance of being as prompt as possible; but there is another, and a very urgent reason, namely, that, if the operation be delayed until intestinal inflammation has been induced within the abdomen, it is far from certain that this inflammation will subside on the removal of the hernia which caused it. I have performed the operation for strangulated hernia, according to the usual mode, a considerable number of times, I believe twenty-three in all, and except in one case, where death occurred in consequence of an attack of phlegmonous erysipelas, which commenced after the patient was considered out of all danger, in every instance with success. This success I attribute to two things, namely, avoiding all undue and useless handling, and performing the operation early. My decided impression is, that the reason why the operation is so frequently followed by death, instead of being one of the most successful of the great operations of surgery, is too great delay in resorting to an operation, and the undue and the injurious use of the *taxis*, even after its adoption has proved unavailing."

But little room is left us for the remaining sections of the volume. Enough has already been submitted to enable any one to form a pretty fair estimate of the general nature and value of its contents. For aught else we must refer inquirers to the book itself, confident that they could not do a wiser thing than to make it for a while their close companion on the study table. A few words of comment more, and we have done. Among the ten concluding chapters may be found an excellent review of calculous disorders in Chapter

15th, which, although previously pretty full for such a text-book, has been considerably enriched through the good offices of Dr. Neill, who has availed himself, to an excellent purpose, of the admirable materials of Dr. Gross. Chapter 17th is deficient in several topics which are too decidedly important to be slurred over or omitted altogether. Nothing, for instance, is said about spasmodic stricture, the pathology of which has been so beautifully and ably demonstrated by Mr. Hancock in his *Lectsonian Lectures*. Nor is much more to be found concerning retention of urine, from any cause, whether stricture, abscess of perineum, rupture or injury of the perineum, enlarged prostate or other of the different local dyscrasies and displacements. These surely are matters of vital moment to practitioner and patient. In fact, there is hardly a class of ill which flesh is heir to that will more certainly fall to the lot of the young surgeon in his practice than these frequently most terrible affections.

Chapter 18th introduces us to Amputations and Resections, respecting which we have only two remarks to make. It is a curious fact that the circular method of amputation, preferred in the great majority of cases in this country, and, if we mistake not, by army surgeons everywhere, is never once alluded to. It seems, under the influence of local prejudices, to have been so completely banished as to be lost sight of altogether; whilst, more oddly still, its absence has escaped even the sharp eye of the American editor.

Our next remark concerns the use of chloroform, in relation to which our editorial friend has, we think, been again at fault. Chloroform appears to be a standing dish—a uniform preliminary and leading measure, not only in amputations and resections, but in dislocations, fractures, strangulated hernias, and retentions of urine also, if not, in short, in nearly all pain-creating manipulations. This resort to anaesthesia by chloroform we know to be the tendency at present ruling among the British surgeons, and above all the Scotch; but it is not so inordinately the practice and feeling amongst us in this country, at least in Philadelphia. We have little fear of anaesthetics, chloroform excepted, and no one is more ready than ourselves to resort to them on proper occasions and with due precautions, for the purpose of relaxing spasm, assuaging the pain of disease, or blunting the agony from operations; but we do not consider this employment of so powerful an agent for good or evil to be a mere matter of routine too simple and trifling in itself to demand the labour of special caution and instruction. Chloroform, at all events, is manifestly nowhere the harmless instrument it might be supposed to be in Mr. Pirrie's cavalier prescriptions of it. Least of all is it reliable in this country, where the article itself may vary in character and poisonous constituents with every laboratory from which it comes, and must in any case, however pure itself, become, in the hands of an ignorant or careless operator, a source of serious injury and danger. There is good reason, therefore, for the regret that neither the author nor editor has thought of giving some definite advice as to its effectual and legitimate uses, or of entering a caveat against its indiscriminate employment by inexperienced or ignorant hands.

The remainder of the volume is taken up with a very good *résumé* of diseases of the eye; with another still better, although briefer, summary of tumours, especially in their histological connections; and finally with some other less interesting topics, upon which we have no time to dwell.

We present an extract from the chapter on Tumours, containing a brief description of cancellous osseous tumours, with a view more particularly to exhibit the two beautiful illustrations drawn from a specimen in the author's own collection:—

"Cancellous bony tumours are generally more or less round in form; though somewhat smooth upon the surface, more frequently present numerous lobes and nodules. They are slow in their growth, but often attain a great size. Mr. Paget mentions that the largest he has had an opportunity of seeing is in the Museum of the Royal College of Surgeons of England. It surrounds the upper two-thirds of the tibia; and measures a yard in circumference. The largest I have seen is one in my own collection, of which the two accompanying figures are good delineations:—

Fig. 288. Front view.

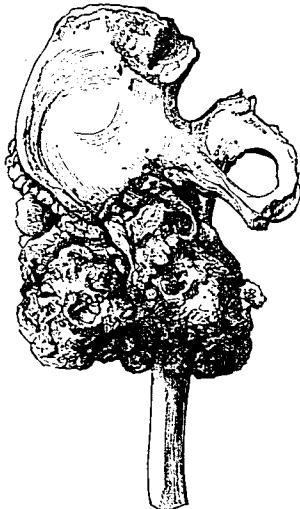


Fig. 289. Back view.



"It is an immense mass of bone attached to the os innominatum, cancellous in its interior, nodulated on its surface, and covered by a thin layer of compact bone. The patient laboured under the disease for ten years, and died in consequence of the sloughing of the soft parts. As the osseous tumour was uncombined with other elements, it cannot be said to be an example of osteosarcoma, under which appellation many include all tumours in which bone is mingled with soft tissue. The viscera were perfectly healthy."

So much for Dr. Neill's edition of Mr. Pirrie's *Elements of Surgery*.

The publishers have done their part so handsomely that, with the aid of the editor's additional text and illustrations, they have really produced a better book than the original. The paper, printing, and engraving are all of the very first class, and entirely worthy of the high position which the work is destined to assume. That an opportunity for the much more material improvement that awaits it may very soon present itself in the call for a new edition is our earnest hope and expectation.

E. H.